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ART. XLIX.—*On a simple and precise method of measuring the wave-lengths and velocities of sound in Gases; and on an application of the method in the invention of an Acoustic Pyrometer;* by ALFRED M. MAYER, Ph.D., Professor of Physics in the Stevens Institute of Technology.

1. *The measurement of the wave-length.*—Without any consideration as to the velocity of sound or the number of vibrations producing a given note, we can accurately measure the wave-length of the note by the following simple arrangement of apparatus, which is an instrumental simplification of the method first used by Zoch (Pogg. Ann., vol. cxxviii, p. 497.)

On the acoustic bellows fix an organ pipe, and place opposite its mouth a Helmholtz resonator responding to its note. Lead from the resonator a thick gum tube to one of König's manometric capsules, whose gas jet is placed near the jet of the organ pipe capsule. Sound the pipe, and by means of the manometric flame-micrometer* adjust the two flames so that their serrations appear to coincide when viewed in a cubical revolving mirror. Now suppose, for simplicity, that the pipe gives 342 complete vibrations in one second; then, taking the velocity of sound at 342 meters per second (at 15° C.), it will require $\frac{1}{342}$ of a second for an aerial pulse to traverse one

* See "On the method of detecting the phases of vibration in the air surrounding a sounding body; and thereby measuring directly in the vibrating air the length of its waves and exploring the form of its wave-surface," Nov. No. of this Journal. In this paper I gave the credit of the suggestion on which I founded my micrometer to M. Radau; but I find that it is due to Zoch (Pogg. Ann., vol. cxxviii). M. Radau mentions it in his *L'Acoustique* without giving credit to the real inventor.

meter. Therefore, if the resonator tube be lengthened $\frac{1}{2}$ meter the serrations of its flame will no longer coincide with those of the pipe, but will bisect the spaces between the latter; for an impulse from the resonator has now to traverse such an increased length that it arrives at its manometric flame $\frac{1}{660}$ of a second later than before the tube was lengthened. If the tube be lengthened one meter, or a whole wave-length (German), the displacement of the resonator serrations will amount to the entire distance separating the centers of two contiguous serrations; and on elongating the tube $n$ number of wave-lengths, $n$ number of such displacements will occur. Thus can be measured, not only *one*, but *many* wave-lengths, for I have not seen sensibly diminished the intensity of the pulses after they have traversed many meters of firm thick tubing. Therefore the error made in the determination of the distance occupied by many wave-lengths will not be greater than that occurring in the measure of the length of only one: and, consequently, this error being divided over so great a number will proportionally increase the accuracy of the deduced length of a single wave.

2. *The determination of the velocity.*—If the number of vibrations given by the pipe can be determined with an accuracy comporting with the above measure of its wave-length, we will succeed in arriving at very precise measures of the velocity ($v=n\lambda$) of sound in air and in various gases. To make the measure of a wave-length in a gas other than air, we close the mouth of the resonator with a delicate membrane, and fill the resonator, its capsule and connecting tube with the gas; or, we can substitute for the resonator a cavity of the proper volume and form, closed by a large membrance which vibrates in unison with the fundamental note of the pipe, and proceed as above.

It requires but little consideration to see that the determinations of the acoustic wave-length and velocity of sound in a gas, by the process I have described, greatly exceed in accuracy the results heretofore obtained by Dulong, Wertheim and others who deduced the length of the wave and velocity from measures of the internodal distances in organ pipes; and I have reason to be of the opinion that my method will give results exceeding in accuracy even those obtained by Quincke (Pogg. Ann., vol. cxxviii, p. 177), who used an instrument which embodies the principle invented by Herschel, and which has its highest development in the exquisite interference apparatus which König has recently described in Poggendorff's Annalen, Bd. cxlvi, p. 165.*

In my lecture-room I have hung up before the students a series of gum tubes having lengths of $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3, etc., wave-

* See the translation of König's paper in this No. of the Journal.

lengths of different notes. The tubes, forming any one of these series, are used with the organ-pipe and resonator corresponding to their note; and as they are successively adapted to the resonator, they cause the serrations of its flame successively to coincide with and to bisect those of the organ pipe flame. Students after such exhibitions do not depart from the room with their usual skepticism as to the existence of an acoustic wave-length, but look upon the tubes as measures of actual entities.

3. *The Acoustic Pyrometer.*—Having devised this simple arrangement of apparatus for measuring the number of wave-lengths contained in a given tube, the idea occurred to me that I could use the method in determining the variation in the number of wave-lengths contained in this tube, caused by a change in the temperature of the air which it contained; and thus succeed in readily determining any temperature to which the tube might be exposed.

The accuracy of this (as far as I know) entirely new method of pyrometry, and the facility of its application can be judged of by the following discussion.

The formula $V = \sqrt{\frac{g h \Delta}{d}(1 + a t)}\frac{c'}{c}$ gives the velocity of sound in air of a known temperature. This formula, as is well known, is reduced numerically to $V = 333^{m}\sqrt{1 + \cdot00367\, t}$. In which, V = the velocity of sound at the temperature $t$ centigrade; 333 = the velocity of sound in meters, at 0° C.; and ·00367 the coefficient of expansion of air under a constant pressure. We will suppose that we have outside of the furnace, whose temperature we would measure, an $UT_4$ organ pipe; that we have placed opposite its mouth an $UT_4$ resonator; and that tubes from pipe and resonator lead to contiguous gas jets placed before the revolving mirror. We will also assume that the air in and around the organ pipe is at 0° C., and that the serrations of the flames of pipe and resonator are brought to coincide when 13 meters of metal tube, connecting the resonator with its manometric capsule, are placed in a furnace which also has the temperature of 0° C. Therefore the length of a wave in the furnace tube is $\frac{333}{512} = 0^{m}\cdot65$, and it will contain 20 wave-lengths. Now gradually raise the temperature of the furnace to 820° C. As the temperature rises we will see the serrations of the resonator flame gradually slide over those of the organ pipe flame, and when the temperature has reached 820° C. we will have observed that the serrations of the resonator flame have glided over 10 times the distance separating the centers of two contiguous serrations of the flame of the organ pipe; for at

820° C. the air in the furnace tube will have expanded to 4 times its volume at 0° C., and therefore $\left(\lambda=\frac{333\sqrt{1+\cdot00367\times820}}{512}\right)$ it will contain $\frac{1}{2}$ the number of wave-lengths it did when at 0° C., and the length of one of these waves in the tube will be 1·3 meter.

We will now determine the limit of accuracy of the method by elevating the temperature of the furnace 100°, or to 920° C. At this temperature the velocity of the pulses in the furnace tube will equal 696·63 meters, and the length of the wave at this velocity will be $1^m\cdot36$. But $1^m\cdot36-1^m\cdot3=0^m\cdot06$, the difference in wave-length produced by the increase in temperature from 820° to 920°, and sufficient to cause the serrations to be displaced ·46 of the distance separating the centers of two contiguous serrations of the organ pipe flame. But by means of the manometric flame micrometer $\frac{1}{10}$th of this displacement can be measured, therefore, we can measure an increase of 10° C. in temperature above 820°.

From an examination of the well established formula for the determination of the velocity of sound, it will be seen that the accuracy of our determinations of furnace temperatures will alone depend on the precision of the coefficient ·00367, which is the number arrived at by Regnault and Magnus for the expansion of air under a constant pressure; and this is one of most reliable constants we have in Physics. Hence the accuracy of our measures, to 10° C., will be equal to those of the air-thermometer, whose indications are, at present, necessarily received as our standards of thermometric determinations.

We will now examine the relation existing between temperatures and wave-lengths. I have computed two tables; the first gives the velocities of sound and the wave-lengths of the note $UT_4$, corresponding to temperatures between 0° C. and 2,000° C.; the second those corresponding to temperatures between 0° C. and −272·48° C.

| Temperature. | Velocity. | Wave-length. |
|---|---|---|
| 0° C. | 333m | 0m·650 |
| 100 | 389·34 | ·760 |
| 200 | 438·53 | ·856 |
| 300 | 482·72 | ·942 |
| 400 | 523·14 | 1·021 |
| 500 | 560·74 | 1·095 |
| 600 | 596·03 | 1·164 |
| 700 | 629·04 | 1·228 |
| 800 | 660·67 | 1·290 |
| 900 | 690·77 | 1·349 |
| 1000 | 719·64 | 1·405 |
| 1100 | 747·38 | 1·458 |
| 1200 | 774·19 | 1·512 |
| 1300 | 799·96 | 1·562 |
| 1400 | 824·94 | 1·611 |
| 1500 | 849·35 | 1·658 |
| 1600 | 872·96 | 1·705 |
| 1700 | 895·97 | 1·748 |
| 1800 | 918·41 | 1·793 |
| 1900 | 940·26 | 1·836 |
| 2000 | 961·70 | 1·878 |

| | | |
|---|---|---|
| 0° C. | 333· | ·650 |
| — 50 | 300·86 | ·587 |
| —100 | 265·00 | ·517 |
| —150 | 223·14 | ·435 |
| —200 | 171·79 | ·335 |
| —250 | 95·60 | ·186 |
| —272·48 | 00·00 | ·000 |

These related numbers I have projected into the accompanying curve, whose abscissas are the temperatures, and whose ordinates are the wave-lengths. This curve, which is the graphical expression of $y=\frac{333\sqrt{1+\cdot00367x}}{512}$ is evidently a parabola, since it has the form $y^2=ax$; and $y$ will equal 0 when $x$ has receded to the point on the axis of abscissas equal to $-272{\cdot}48°$ C., which is "the absolute zero" of temperature.

It is evident that this same curve will give the numerical relations between temperatures and the wave-lengths of any note, or the velocities of sound in any gas, by merely giving different numerical values to the divisions on the axis of ordinates.

It only remains to give the simplest formula for determining the temperature of the furnace in terms of the observed displacements of the resonator serrations, and of the known number of wave-lengths in the furnace-tube at the temperature $t$.

Let $t$ = temp. C. of the air in and around the organ-pipe.
$t'$ = " " " the furnace-tube.
$v$ = velocity of sound at temp. $t$.
$v'$ = " " " " $t'$.
$l$ = number of wave-lengths in furnace-tube at temp. $t$.
$d$ = observed displacement of resonator serrations by an elevation of temperature $t'-t$.

Then $l-d$ will equal the number of wave-lengths in the furnace-tube (allowance made for elongation of tube by heat) at the temp. $t'$. As the velocity of sound in the furnace-tube will be inversely as the number of wave-lengths it contains, it follows that

$$v' : v :: l : l-d; \text{ hence } v'=\frac{v\,l}{l-d}; \text{ but}$$

$$(1) \qquad v=333\sqrt{1+\cdot00367t}, \text{ and}$$

$$(2) \qquad v'=333\sqrt{1+\cdot00367t'}, \text{ hence}$$

$$(3) \qquad \frac{v\,l}{l-d}=333\sqrt{1+\cdot00367t'}$$

Reducing equation (3) we obtain

$$(4) \qquad t'=\left(\frac{v\,l}{20{\cdot}16\,(l-d)}\right)^2-272{\cdot}48,$$

which gives $t'$ in terms of $v$, $l$ and $d$. Combining equations (1) and (3) we obtain

$$(5) \qquad t'=\frac{272{\cdot}48\,(2l-d)\,d+dl^2}{(l-d)^2},$$

which gives $t'$ in terms of $l$, $d$ and $t$. But as $v$ has to be calcu-

lated in order to obtain $l$ in equation (5), it follows that equation (4) is the simpler and the more readily worked numerically.

If we call T *the absolute temperature* centigrade, then $T=t'+272{\cdot}48$, and equation (4) becomes (6) $T=\left(\frac{v\,l}{20{\cdot}16\,(l-d)}\right)^2$, in which equation the origin of coördinates is at the vertex of the parabola.

This paper is intended only to give a general account of the new method of pyrometry, and therefore I have not touched on the details of apparatus and experiment. These I will present in a subsequent communication, in which accounts of actual applications of the method will be given. I may, however, here remark that, if the tube in the furnace is 13 meters long and of $0^m{\cdot}015$ diameter, it can be coiled into a spiral of $0^m{\cdot}5$ in diameter, or into two spirals, each of $0^m{\cdot}25$ diameter. Or, the tube can be bent on itself several times and thus form a compact fascicle of tubing. Also, the tubing should *ascend* from the resonator to the furnace-tube, and *descend* from the latter to the manometric capsule, so that the rarefied air in the hot tube cannot enter the tubing outside of the furnace. If the serrations of the manometric flames are too dim to be readily observed, they can be rendered distinctly visible, even in broad day-light, by the use of "carbonized" gas, or by sifting into them the fine scrapings of lead pencil. In ascertaining the number of displacements produced by any temperature, the furnace-tube is slowly moved into the furnace, so that the displacements of the resonator serrations can be counted as the tube gradually attains the highest temperature, when the serrations become stationary.

October 12, 1872.

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ART. L.—*On the stability of the Collodion Film;* by LEWIS M. RUTHERFURD.

THE very numerous and concordant micrometric measures of star photographs, made by myself and under my direction during the past seven years, have inspired me with the utmost confidence in the stability of the collodion film, particularly when applied to a plate of glass properly albumenized.

The measures above referred to were made upon nearly thirty plates of the Pleiades group taken at many different times, and embracing within a space of about 80′ square from thirty to seventy-five star images, according to the time of exposure, the state of the atmosphere and the sensibility of the chemicals; also upon many plates of the Præsepe group, aver-

aging about thirty-four stars on each plate; also upon many plates of the group about $\theta$ Orionis; also upon many plates of the group surrounding 41 Boötis; also of the group surrounding $\mu$ Cassiopæa.

In addition to the measures of the position and distance of the members of the several groups mentioned, very many measures have been made upon long lines of star images, at intervals of one second of time from each other, for the purpose of determining the angular value of any given portion of the plates. Very many measures have been made for the same purpose upon selected star pairs, and the results of these measures compared with the results of transits of stars over diamond lines drawn upon a plate of glass placed in the photographic plate-holder at the photographic focus,—and finally many measures have been made upon star groups taken on the preceding, central and following regions of the plates for the purpose of detecting the character and amount of the distortion, if any, of the images.

The results of all these measures were so concordant as to forbid the idea of the existence of any great change in the collodion film, and the conviction of this stability is very greatly strengthened by a very few attempts to remove the collodion film from an albumenized plate by any ordinary mode of rubbing or washing.

This confidence in the reliability of the photographic method for measures of precision was rudely shocked by the statements of Mr. Paschen, contained in an elaborate article upon the application of photography to the observation of the coming transit of Venus, printed in the Astronomische Nachrichten in April last. He found in three measures upon albumenized plates and upon one not albumenized respectively, a shrinkage to the following fractions of the whole space measured: in No. 1, $\frac{1}{323}$, No. 2, $\frac{1}{618}$, No. 3, $\frac{1}{1836}$, No. 4, $\frac{1}{31233}$. Nos. 1 and 2 were measures of the same plate, but in directions perpendicular to each other. No. 4 was on a plate not albumenized: these quantities are so large and irregular, that if they really exist, we must be compelled to relinquish the hopes based upon the use of photography for precise astronomy. My own experience and a distrust of the methods used by Mr. Paschen for obtaining these results, prevented me from losing confidence in the value of a photographic image. Still the question was so grave and the general merits of Mr. Paschen's article were so great, that I determined to subject the point to a thorough examination, and for that purpose the following measures were made upon plates, albumenized before the application of the collodion, first when leaving the camera and quite wet, and second when they had become dry; some of the plates were neg-

atives of dark lines upon a white ground, and some of white lines upon a dark ground; in some while yet wet most of the collodion between the lines was removed to encourage a shrinkage outward, and in others most of the collodion outside of the lines was removed to encourage a shrinkage inward; most of the collodion used was such as is in ordinary use in my observatory. The measures were made by the aid of my micrometer screw, one revolution of which is equal to $\frac{1}{4\,0}$ of an inch. The plates were made and the measures conducted by Mr. Chapman, my assistant, who is an accomplished photographer and familiar with the use of the micrometer. The plates were left clamped upon the stage of the micrometer during the interval between the measures wet and dry, and both these measures were consequently made on the same parts of the screw. In every case five bisections of each of the two lines were made, and the difference between the means of these readings shows the distance between the lines in terms of screw revolutions. For the purpose of exhibiting the degree of precision attainable by the measures, I will transcribe in full the measures of the first two plates, giving for the rest only the results.

The values of the measured spaces have been reduced to a uniform temperature of the screw of 68° Fahrenheit by the application of a correction deduced from the recognized coefficients of expansion of steel and glass. It will be seen that in all cases save two the distance was greater between the lines when the plate was dry than when wet, the mean excess of the nine measures is Rev. 0·0017, which is $\frac{1}{5\,8\,8}$, $\frac{1}{2\,3\,5}$ of an inch; it reaches in no case $\frac{1}{6\,8\,8}$ $\frac{1}{6\,8}$ of an inch. This result is no doubt due to the cooling of the glass plate, by the evaporation which takes place the moment the wet plate is taken from the plate-holder and exposed to the air under the micrometer. This excess of distance (Rev. 0·0017), would be caused by an increase of temperature for the dry glass of about 4° F.

This consideration reveals a source of error in the use of wet plates which I have not hitherto considered, since the same evaporation takes place no doubt during the long exposures given to star-plates; the amount will vary according to the hygrometric state of the atmosphere, and may be met by reading wet and dry bulb thermometers.

My objection to the method used by Mr. Paschen, as I understand it, is that instead of being confined to an investigation of what happens to the collodion film between the moment of exposure wet and the moment of measurement when dry, it is a comparison of the actual state of the plate when dry, what it ought to have been had all the adjustments, manipulations and instruments been perfect.

| Plate No. 1. June 27, 1870. Therm. 74°. | | Plate No. 2. June 27, 1870. Therm. 74°. | |
|---|---|---|---|
| *Wet.* | | *Wet.* | |
| Line No. 1. | Line No. 2. | Line No. 1. | Line No. 2. |
| 165·5500 | 72·1175 | 164·3275 | 71·9350 |
| ·5470 | ·1125 | ·3325 | ·9450 |
| ·5525 | ·1225 | ·3250 | ·9300 |
| ·5575 | ·1275 | ·3350 | ·9370 |
| ·5540 | ·1240 | ·3275 | ·9475 |
| 165·5522 | 72·1208 | 164·3295 | 71·9388 |
| 72·1208 | | 71·9388 | |
| 93·4314 | | 92·3907 | |
| +10 Therm. Cor. | | +10 Therm. Cor. | |
| 93·4324 | | 92·3917 | |
| *Dry.* | Therm. 72°. | *Dry.* | Therm. 72°. |
| 165·1125 | 71·6850 | 163·9075 | 71·5100 |
| ·1150 | ·6880 | ·9000 | 50 |
| ·1175 | ·6890 | ·9100 | 00 |
| ·1175 | ·6850 | ·9025 | 50 |
| ·1125 | ·6860 | ·9025 | 50 |
| 165·1150 | 71·6866 | 163·9045 | 71·5130 |
| 71·6866 | | 71·5130 | |
| 93·4284 | | 92·3915 | |
| +7 Therm. Cor. | | +7 Therm. Cor. | |
| 93·4291 | Dry−R.0·0033 | 92·3922 | Dry+R.0·0005 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Plate | No. 1. | June | 27. | Therm. | 74° and 72° | Dry− | R. 0·0033 |
| " | No. 2. | " | " | " | 74° and 72° | " + | R. 0·0005 |
| " | No. 3. | July | 3. | " | 82° and 83° | " + | R. 0·0004 |
| " | No. 4. | " | " | " | 84° and 85° | " + | R. 0·0007 |
| " | No. 5. | " | 8. | " | 77° and 78° | " + | R. 0·0009 |
| " | No. 6. | " | " | " | 82° and 80° | " + | R. 0·0092 |
| " | No. 7. | " | 10. | " | 78° and 80° | " − | R. 0·0059 |
| " | No. 8. | " | " | " | 81° and 83° | " + | R. 0·0040 |
| " | No. 9. | " | " | " | 84° and 82° | " + | R. 0·0089 |

Mean excess of Dry, R. 0·0017=$\frac{1}{588.236}$ of an inch.

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ART. LI.—*Note upon Aventurine Orthoclase, found at the Ogden Mine, Sparta Township, Sussex Co., N. J.;* by Prof. LEEDS.

AMONG the masses of gneiss rock thrown out in sinking one of the shafts of the Ogden mine, I found, during the course of the past summer, large quantities of very beautiful sunstone, which appears hitherto to have escaped notice. The three cleavages *O*, *i-i* and *i-i*, are easily obtained, and afford the cleavage angles of orthoclase. The very thin plates, which may be procured by slicing the stone in the direction of the principal

cleavage, are of considerable size, and furnish excellent specimens for microscopic examination.

The color of the orthoclase is a delicate flesh-red, which color is due entirely to the imbedded crystalline scales of what has been supposed to be göthite. The stone itself is translucent and quite colorless. The results obtained in two analyses were:

| | 1. | 2. | Mean. |
|---|---|---|---|
| Silica, | 64·80 | 64·82 | 64·81 |
| Alumina, | 19·02 | 19·25 | 19·02 |
| Ferric oxide, | 0·23 | | 0·23 |
| Lime, | 1·29 | 1·23 | 1·26 |
| Magnesia, | 0·61 | 0·58 | 0·59 |
| Potash, | 15·22 | 13·38 | 14.30 |
| Ign., | 0·26 | 0·26 | 0·26 |
| | | | 100·47 |

In an analysis of an aventurine oligoclase from Tvedestrand in Norway, Scheerer obtained $SiO_2$ 61·30, $Al_2O_3$ 23·77, $Fe_2O_3$ 0·36, CaO 4·78, $Na_2O$ 8·50, $K_2O$ 1·29. In this the per cent of göthite is somewhat greater than in the New Jersey orthoclase, but in both cases the extremely small amount of foreign matter which suffices to impart the brilliant aventurine character to the feldspar is remarkable. It is worth noting in this connection that all the specimens of sunstone from Kennett, Chester Co., Pa., in the cabinet of the Stevens Institute, are oligoclase, not orthoclase.

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ART. LII.—*On Soil Analyses and their Utility;* by EUG. W. HILGARD, State Geologist of Mississippi.

(Read at the Dubuque Meeting of the Am. Assoc. Adv. Sci., August, 1872.)

IN the American Journal of Science for September, 1861, Prof. S. W. Johnson published a criticism on the "Soil-analyses of the Geological Surveys of Kentucky and Arkansas," whose strictures, to a great extent eminently just, appear to have so impressed the scientific public in this country, that few if any soil-analyses have since then been made in connection with any state or national survey, excepting that of the State of Mississippi, where the work already begun was continued, either by myself, or under my charge, or recommendation, by others. Holding myself responsible for this departure from the generally adopted views, I propose in the present paper to discuss specially Prof. Johnson's objections, and to give my reasons for persisting in a course of research that has, more than once, secured for myself and my co-laborers the compassionate sym-

pathy of true believers. While I consider the work far from being as complete as it should be, and for that as well as other reasons its publication in detail may be delayed for some time; yet I think what can now be said of sufficient importance to be brought before this meeting.

I propose, in this discussion, to maintain the mainly practical standpoint assumed by Prof. Johnson himself. I shall therefore leave out of consideration the performance of such exhaustive investigations of *all* the physical and chemical properties of the soil, as have been made in some cases, for special purposes, e. g., by Prof. Mallet, on some of the cotton soils of Alabama. If the investigation of each soil, to possess practical importance, requires from three to six months labor, we may as well, for practical purposes, consider such researches out of the question for the present. We want something analogous to the metallurgical assay of minerals, as distinguished from their complete ultimate analysis. So far, therefore, as the agricultural qualities of a soil may be inferred and approximately estimated by an experienced eye, I would relieve the chemist from the exact numerical determination, e. g., of the power of absorbing heat from the sun, the specific heat, the "water-holding" power, the capillary coefficients, etc. However necessary for theoretical investigations, I hold that for practical purposes, these laborious determinations may in most cases be dispensed with; since from what has already been done, or what can be done with a few typical soils, we may infer the comparative magnitude of these coefficients with a sufficient degree of approximation.

The amount of labor bestowed on each soil by Dr. Peter, as reported in the Kentucky and Arkansas surveys, approaches very closely the limit beyond which the *immediate* advantages to be derived from such knowledge of soils as analysis may impart, would seem, to many, disproportioned to the expenditure involved. How very modest we are, truly, when a purely scientific object is involved, whose immediate practical application is not obvious at a glance! In what other branch of technical science would it be thought admissible to proceed without obtaining such knowledge of the prime materials as chemistry may afford, even if *no* immediate application of this knowledge be foreseen? Our public treasuries are constantly drawn upon for hundreds of thousands of dollars, in behalf of objects of at least questionable usefulness. Yet Prof. Johnson seems to have thoroughly satisfied our state geologists that they are not justified in giving the virgin soils of their respective States the benefit of such light as chemistry may even now confessedly afford; apart from the important general inferences which may fairly be expected to be drawn hereafter from the history of their cultivation. How are we to advance in our knowledge of

soils, if we abandon as hopeless the determination of their chemical character? Are the proofs that have been brought against the utility of soil analyses really of such a character as to justify so grave an omission? an omission, too, which in many cases cannot hereafter be supplied. Even in the comparatively youthful State of Mississippi, I have found difficulty in obtaining reliable specimens of some soils, whose great productiveness had led to their cultivation by the earliest settlers, over the entire area of their occurrence.

I question the propriety of this omission, and the justice of the *testimonium paupertatis* thus inflicted upon agricultural and analytical chemistry.

To define my position, I premise that—

1. I fully agree with Prof. Johnson as to the comparative uselessness of a single analysis giving the percentages of soil ingredients found, *in ordinary cases.* It is only when such analysis demonstrates the *great abundance*, or *very great deficiency*, of one or several primarily important ingredients, that, by itself, it conveys information of considerable practical importance. Note, that such cases are not altogether infrequent, even in virgin soils.

2. I agree that an "average soil" is a *non ens*, except as referred, comparatively, to a *particular set of soils closely related in their origin.*

3. Also, that the claim of being able to detect the minute differences caused by cropping without return to the soil, is precarious, and perhaps beyond the power of our present analytical resources.

4. I further admit that, ordinarily, the analysis of soils long cultivated, and treated with manures, can give but little and very partial information as to the condition and composition of the soil; from the great difficulty, if not impossibility, of obtaining fair representative specimens.

5. Furthermore, that to designate soils by the names of the Cretaceous, Carboniferous or Silurian strata they may happen to overlie, is very loose practice; since in most cases they are derived from Quaternary deposits, which may or may not have been influenced in their composition by the subjacent rocks.

On the contrary I demur, in the first place, to the broad assertion that "it is practically impossible to obtain average specimens of the soil," as inapplicable to a very large class, especially of virgin soils, covering large areas with a uniformity of character corresponding to that of subjacent formations, from which they have been directly derived, by substantially identical and uniform, or uniformly variable, processes.

The importance of this exception is not, it is true, very obvious in the stony fields of New England (such as discouraged

Prof. Johnson in his vacation trip to Northern New York), or in fact, in any district where a great variety of formations has directly contributed toward forming the soil, and "chunks" of undecomposed minerals are diffused through it. In such cases, the analysis of the rock which has predominantly contributed to the mass of the soil proper, would be a more correct index of the prevalent characteristics of the latter, than if itself were taken in hand. And from such analyses we could at least deduce what ingredients, and in what form, it would certainly be *useless* to add to the soil.

But when we come to the great plains of the West and Southwest, whose soils are consistently derived from widespread Quaternary deposits, composed of materials almost impalpable save as regards siliceous sand; or even the rolling uplands of the Gulf States, whose subsoil stratum of "yellow loam" can only be diluted, but not otherwise changed, by the admixture of the underlying drift, leached long ago of everything soluble in carbonated water, or available to plants: the objection based upon the supposed impossibility of securing representative specimens, becomes obviously untenable; as I shall hereafter show from the close correspondence in the composition of soils, and especially *sub*soils, from widely distant portions of the State, derived from the same geological (Quaternary) stratum.

A word in regard to the "freaks and accidents" mentioned by Prof. Johnson as liable to make sport of the devoted analyst. Undoubtedly such errors must be ultimately provided against by multiplication of analyses (not necessarily of the same acre, but of other corresponding specimens, in the sense mentioned above); and while questioning the efficiency of a bird or squirrel in vitiating a properly taken sample of soil, I must admit the disastrous consequences which might result if a dog, cow, or horse were similarly concerned. No specimen of "virgin soil" can, of course, be obtained where such animals usually do congregate. But as a rule, it is not at all difficult to avoid such places; while the chance of accidentally hitting upon a sporadic animal deposit in the broad woods or prairies is singularly small, and is notably diminished by the circumstance, that an attentive observer (and none other should take soil specimens) will be able to distinguish such localities for years, by the peculiarity of their vegetation.

I will remark, however, that I consider the sampling of a soil with a view to securing a representative specimen, as a matter second in difficulty and delicacy only to the analysis itself; that I rarely have thought it worth while to analyze specimens sent by other than intelligent persons specially instructed by me; and even then have frequently had to reject them, from

their having obviously been taken at an improper locality, e. g., near a foot-path, by the side of a fence, on a partially denuded hillside or ravine, in the bed of a run, at the foot of a tree, etc.

The question of *depth* must, in my view, be left to be determined by the circumstances of each case, except in so far as the extreme depth to which tillage may cause the roots of crops to reach, must be within the limits of the samples taken. Of these, *one* should ordinarily represent what, under the usual practice of tillage, becomes the arable soil; another, the subsoil not usually broken into; a third will in most cases be useful to show what materials would be reached were the land to be underdrained. As a rule, I have taken no specimens of soil to a less depth than six inches, and as much deeper as uniformity of color reached—for obvious reasons. But in special cases, when important differences were suggested by the aspect of the soil and subsoil, they have been separately examined, at whatever depth the change of color might occur.

With soils of the character referred to, samples selected and taken with due care, and strict attention to thorough intermixture, both in the field and subsequently in the laboratory, I am unable to see why even two grammes may not correctly represent the characteristics of a 1000 acre tract. Not that every point of that tract would be likely to give the same percentage result, perhaps; especially as regards the surface soil, which might in places be more clayey or more sandy than the sample analyzed. Still, the *relative proportions of the soil ingredients, and their degree of availability*, would remain substantially the same; the wider range and readier penetration of roots in sandier soils, making up, within certain limits, for the smaller percentage of available ingredients in a given bulk, as compared with more clayey ones.

From the fact that the atmospheric surface water must, in its course, inevitably have a tendency to bring about such inequalities, by carrying forward the finer particles of the soil in larger proportion than the coarser ones; as well as from the greater influence of vegetation: we shall, in the series of analyses made a postulate by Prof. Johnson, expect to find a closer agreement between those of subsoils than those of surface soils. Such I find to be very decidedly the case; so much so, that I habitually look to the former as the most reliable index of a soil's distinctive character. To this there can be no legitimate objection, when, as in all the upland soils now under consideration, the surface soil is directly derived from the subsoil, and its depth is less than thorough culture would give to the arable soil.

As regards the analysis itself, I premise that I have always found even the most "chemically pure" reagents sold by dealers quite inapplicable to the purpose of soil analysis. From first to last, I have prepared or purified these myself; and, as regards the acids, especially hydrochloric, I have found it necessary to reject, as a rule, even the purest, after keeping it for a few weeks in a glass bottle. The same is true, and perhaps in an aggravated degree, of aqua ammoniæ. The severe ordeal of slow evaporation on a bright platinum foil will rarely be passed by ammonia a fortnight old; and still less frequently by hydro-sulphide of ammonium.

Armed with these, and a multitude of other precautions, usual and unusual, to secure the utmost possible accuracy; always treating the soil with the same large excess of acid of uniform strength, and precipitating all corresponding precipitates as much as possible from the same volume of liquid; using none but the best Bohemian glass, and platinum vessels, and filters specially extracted—operating, in short, as uniformly as the nature of the materials would permit: I confess I felt considerable confidence in the correctness of my results, until the experiments made in Bunsen's laboratory, on the solubility of glass vessels, gave rise to unpleasant doubts. On consideration, however, I found that the (sensibly constant) error so introduced would not, when allowed for, amount to more than the differences between two analyses of one and the same material, or vitiate in any serious degree the conclusions arrived at. Nevertheless, I shall hereafter, to the utmost possible extent, carry on all operations liable to introduce errors on this score, in platinum and porcelain vessels, as advised by Bunsen.

As regards Dr. Peter's failure to determine the amounts of soluble silex, nitric acid, ammonia, chlorine, and the degree of oxidation of the iron, I agree that the former is desirable, not only because, whether "essential" or not, some plants do habitually absorb it in very large quantities, and it might be best to let them have it; but also because it is a desirable index of the degree of decomposition which the soil silicates have undergone. I have therefore made this determination regularly, by boiling with solution of sodium carbonate. In a series of these determinations, an unmistakable relation between the soluble silex and the amount of lime in the soil becomes manifest; as might, indeed, have been foreseen.

As regards nitric acid, the consideration suggested by Prof. Johnson himself, viz., that its quantity must be exceedingly variable, within short periods, in one and the same soil, seems to me a sufficient dispensation from the laborious determination.

The same holds good, in a measure, for ammonia. Its quan-

tity varies continually in the soil, as it does in the atmosphere; its chief absorbers in the soil are "humus" and clay. Where these prevail largely, ammonia can scarcely be deficient as a nutritive ingredient to an injurious extent; albeit, more might doubtless be beneficially added. Moreover, the characteristic effects of ammonia on vegetation are sufficiently obvious (in "running to weed") to render its determination in virgin soils, laborious and even uncertain as it is, a matter of comparatively little *practical* consequence, however great might be its theoretical interest.

As for the determination of the degree of oxidation of iron, I confess I fail to see its practical bearing. When ferric oxide is present, plants surely can have no difficulty in reducing the modicum they need to a soluble condition. When ferrous oxide exists to any great extent, it indicates a want of drainage, and manifests itself both in the color of the soil and in the poisonous effect on vegetation. But farmers surely do not need the aid of chemical analysis to tell them that their soil needs drainage and aëration! A determination made to-day would be of no value to-morrow, if the soil had been plowed in the interval.

Finally, Dr. Peter *does* determine *chlorine*, in the treatment of soils with carbonated water; though it is not put down in the general analysis. However, the soluble chlorides, like the nitrates, are so constantly liable to variation and, as experience shows, so little likely to be deficient in the soil, that its omission would not be a serious practical objection.

A much graver defect is the failure to determine separately the organic matter ("humus") and the chemically combined water; and to this is owing, in a measure, the unsatisfactoriness of the analyses as regards information on the physical character of the soils. A large amount of water of hydration indicates, in ordinary cases, a correspondingly clayey soil, where heaviness in working may, or may not, be relieved by a large amount of "humus." The "volatile matter" item, however, gives us no information whatsoever on these vitally important points; and there is, unfortunately, no *simple* method by which the determinations in question can be effected even approximately. That they *should* form part of every soil analysis, is obvious, if only on account of the importance of "humus."

I have attempted to obtain a reliable scale of the different degrees of "heaviness" of soils, from the determination of their maximum absorption of hygroscopic moisture at ordinary temperatures. I find that at temperatures from about +7° to +21°, the amount of aqueous vapor absorbed by a thin layer of soil exposed to a *saturated* atmosphere remains very nearly constant, being for

| | | | |
|---|---|---|---|
| Very sandy soils, ..... | 1·5 to | 2·0 | per cent. |
| Loam soils, ..... | 5·0 to | 8·5 | " |
| Clay soils, very heavy, ..... | 12·0 to | 15·0 | " |

there being, of course, all intermediate grades of hygroscopic power, as well as of "heaviness." It appears that for this interval of temperature, the decrease of absolute absorbing power in the soil, resulting from the rise of temperature, is just balanced by the increased amount of vapor diffused in the air—not an unimportant circumstance, with regard to vegetable life.

There are, however, two soil ingredients which interfere seriously with the correctness of the estimate as to "heaviness," derived from the coefficient of absorption, viz., "*humus*" and *ferric oxide*. Both of these are highly hygroscopic, yet both *counteract* the "heaviness" caused by excess of clay. Moreover, there is a class of soils (viz., fine siliceous silts) whose exceeding "heaviness" in cultivation is much complained of, yet whose absorbent power is very small.

When, as in the majority of cases, the surface soil has been directly derived from the subsoil, the disturbing effect of the "humus" may be sensibly eliminated by comparing, not the soils, but the subsoils, in this respect.* As to the ferric oxide, there are among about 200 Mississippi soils analyses but three or four whose agricultural qualities would have been seriously under-estimated by a reliance upon the coefficient of absorption alone.

But I do not for a moment admit, that in a material so complex both in its composition and mode of action any one or few data, whether chemical, physical, or agricultural, may be relied upon to characterize the soil: or, as Prof. Johnson expresses it, "to do violence to agriculture." So far from this, I consider that a proper interpretation of the analytical results must take into consideration, not only all the chemical and physical facts observed on the specimen, but all that has been or can be observed *in loco*—the location, depth, derivation, relations to drainage, etc.; as well as all that is known concerning the qualities or peculiarities of the soil, both in its natural state and in cultivation. As Prof. Johnson says, it should "form part of a system of observations and trials; must be a step in some research; must stand, not as an index to a barren fact, but as the revelator of fruitful ideas."

Such, precisely, has been my object from the beginning of my researches on the soils of Mississippi, for sixteen years past. Clearly, the difference between Prof. Johnson's position and mine is one of degree only; yet this difference is not a slight

* In such cases, the surface soil is always more sandy than the subsoil.

one, since while, as before remarked, I have made, or caused to be made, some 200 analyses of soils and subsoils, his classic works on the growth and nutrition of plants do not contain so much as a tabular exemplification of the composition of various soils, as resulting from chemical analysis. If, then, "the probabilities of its uselessness in direct application to practice are so great," as Prof. Johnson seems to hold, I have committed a grievous error, and squandered the substance of the State.

I think that the considerations already adduced should plead measurably in extenuation of my course. But I will now state succinctly what services, in my view, soil analyses may fairly claim to be capable of performing, when conducted substantially in the manner, to the extent, and under the conditions defined above.

I take it for granted that, if in the determination of the mineral ingredients we were able to distinguish clearly from one another the portion immediately available to plants, from that which is in an unavailable form, we would go far toward accomplishing what was originally claimed for soil analysis; and this Dr. Peter attempted to do by treatment of the soils with carbonated water. It cannot be doubted, however, that plants, as well as agriculturists, have at their disposal much more powerful, or at least more *energetic*, solvents; and that, therefore, a determination of those ingredients which may fairly be considered practically within the reach of agriculture, must go deeper than does that with carbonated water.

Opinions may differ widely as to the proper strength and nature of the solvent ("*Aufschliessungsmittel*") to be selected. Hydrofluoric acid, or ignition with the alkaline earths, would evidently go too far; as no soil, probably, will ever yield up the whole of its nutritive ingredients to plants, and fertility is far from being proportional to the *whole* amount of potash, phosphoric acid, etc., contained therein.

When, however, a *partial* solvent of uniform strength is used in all cases alike, and its action continued for the same length of time, it may fairly be presumed that, *as between soils of similar origin*, the amounts so rendered soluble are, in a measure, proportional to the amounts of available nutriment present.

In using hydrochloric acid of the strength 1·11 to 1·12 sp. g., obtained by slow steam distillation of stronger or weaker acid rejecting the first and last portions, I have in most cases found quite a satisfactory agreement between the results so obtained and the experience of cultivators as to the productiveness and duration of the respective soils; always *provided*, that the difference in the amounts of inert sand present, of specific gravity, of depth of soil, etc., were taken into account

The proviso is important; but that with a proper local knowledge these allowances *can* be made, and that in most cases the information thus gained regarding the nature and treatment of the soil will be vastly more complete and reliable than the judgment of any number of "old intelligent farmers," my experience has fully convinced me; witness the egregious mistakes daily made by such in the selection of new lands. Moreover, a small minority only of farmers is likely to possess the requisite "age and intelligence"; and it is quite important that the multitude of those less fortunate should have the benefit of all the help science can give them.

I will adduce but one "odious example" of a widely prevalent error in reference to the character of a class of soils, that I have as yet been unable to eradicate, even from among the "old and intelligent;" who are unfortunately very much given to theorizing on inadequate premises. Our prairie soils are notoriously limy; they are also very "sticky"; and the mud takes the hair off the feet of cattle. *Ergo*, every "sticky" clay soil in the State is called, considered, and treated as a "prairie" soil, especially as the hardened clods adhering above the hoofs of cattle should carry the hair with them. If such soil is unthrifty, and refuses cotton, it is because "there is too much lime in it," which "scalds" the seedlings. In matter of fact, most of these soils are notably deficient in lime, so as to be most directly and immediately benefited by its application wherever it has been tried, in accordance with my suggestion. The lime here acts, probably, as much chemically as physically; the clay being rich in potash, as per analysis.* While the physical defects of these soils are doubtless the main cause of the crop failures, yet analysis has suggested a remedy which relieves, for the time being, from the necessity of the more costly improvements; lime being comparatively easy of access.

Analogous cases are far from infrequent, both in this and in the adjoining States; and I have been led to attach special importance to the determination of *lime* in soils, from the (not unexpected) rule which seems to hold good very generally, viz., that, *cæteris paribus*, the *thriftiness* of a soil is sensibly dependent upon the amount of lime it contains; while, at the same time, in the usual mode of culture without return to the soil, the *duration* of fertility is correspondingly diminished, and its cessation is very abrupt wherever much lime is present.

It may be said that, after all, this is but what, from data already known, might have been expected. Granted; then, *a fortiori*, soil analysis, involving the determination of lime,

* See, for example, the article "Heavy Flatwoods Soil," in my Miss. Rep., 1860, pp. 276, 279.

is of considerable use in determining the present and future value of soils.

In speaking of the "amount" of lime, I must be understood to refer, not so much to its absolute percentage, as to its quantity in comparison with that of potash, which, with phosphoric acid, is what all our fertilizers chiefly aim to supply. Their determination must, of course, be considered of prime importance, since their absence or extreme scarcity is fatal to profitable fertility; while, when they are present, even though *immediately* available for absorption to a slight extent only, we possess in lime, ammonia, etc., and the fallow, ready and powerful means for correcting their chemical condition.

Here again, the practical value of soil analysis is direct and indisputable. It is of no small interest to know whether the field we intend to cultivate contains 0·75 per cent of potash and 0·25 of phosphoric acid, soluble in H Cl, or only the fifth or tenth part of these amounts. *One* will bear improvement of all kinds—will pay for underdraining, terracing, etc.; while the other, quite similar in aspect perhaps, would not, according to Liebig's testimony, ordinarily be capable of profitable culture.

Again, it is well known that the same species of plants may occupy soils of widely different quality and value. True, an attentive observer will in such cases see differences in the mode of development;* yet these are often such as to escape ordinary remark, and grievous disappointments frequently arise from this source, with new settlers especially. It is of no small importance to be able to *identify*, as well as to distinguish, soils resembling each other; and this, soil analysis can undoubtedly do, if there is any virtue in the law of probabilities even—admitting all that may otherwise be said against their reliability.

Even if no other direct benefits than those already mentioned could be attained by the chemical and mechanical analysis of soils (which I do not admit, and expect to prove otherwise hereafter); even if we leave out of consideration the addition to our general knowledge which may fairly be expected to result from extensive series of such investigations, carried out upon a uniform plan, whereby accidental errors (whether caused by "birds or squirrels," or analytical and other mistakes) will be eliminated; even thus, I contend that the practical and theoretical value of soil analyses is sufficiently great to justify whatever labor and expenditure may be bestowed upon them by state and national surveys; and that the neglect with which this branch of research has

* Miss. Rep., 1860, p. 203.

of late been customarily treated, is the more to be regretted as no probable amount of private effort can accomplish what must, of necessity, be done on an extended scale and with the *prestige*, voluntary assistance, and interest, not usually accorded to any but public enterprises. And with due deference to the author of the two volumes whose extraordinary merits no one appreciates more than myself, I call upon my colleagues in State surveys, especially in the West and South, to reconsider this subject before it is too late, and a legislative fiat declares their work to be "finished." It is true that the agricultural colleges must and will take up and continue, as far as possible, the investigation of the agricultural peculiarities of each State; but the special and local experience acquired by those conducting a field survey, as well as their opportunities for extensive and comparative observation, are unfortunately "not transferable," even to the finest quarto report. In order to attain their highest degree of usefulness, our agricultural colleges should teach, not merely general principles, together with a sufficiency of the handicraft of agriculture; but they should be enabled to point out to each student, with reference to his particular neighborhood, How Crops Grow, and How Crops Feed.

Univ. of Miss., July, 1872.

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ART. LIII.—*The Heat produced in the Body, and the effects of Exposure to Cold;* by JOHN C. DRAPER, M.D.

THE following results were obtained in an attempt to determine the quantity of heat passing off from the surface of the body, by finding how much it would elevate the temperature of a known mass of cool water during a given period of time.

The manner of experimenting was as follows:—Seven and a half cubic feet of cool water were drawn into a bath, and the temperature taken after careful mixing. The bath was then covered over for about four-fifths of its extent to prevent the action of currents of air, and at the close of an hour the temperature was again tested. The rise of $\frac{1}{2}$ a degree represented the amount of heat absorbed from the air during one hour, and was deducted as a normal error from the results afterward obtained.

During the time occupied in determining the normal error of the bath (viz., one hour), I lay on a sofa to bring the circulatory and respiratory functions into a condition similar, as regards position of the body, to that to which they would be submitted

while in the bath. My dress during this phase of the experiment consisted of a thin flannel summer undershirt, linen drawers, and cotton socks. At the completion of the hour of rest these were removed with as little exertion as possible and I stepped into the bath, and lay down, allowing only the head to project above the surface of the fluid. At the close of an hour the temperature of the bath was again taken. I then left it, and drying the surface of the body, reassumed the same dress and lay down on the sofa. Throughout the whole of each experiment, the temperature of the air, the dew point, the temperature of the bath, of the armpit, mouth and temple were taken, together with the rate of respiration and of the pulse.

Since in these experiments two series of phenomena are investigated, I have for the sake of clearness of description separated the results in accordance with the phenomena in question, and direct attention first to the

*Quantity of heat evolved from the body.*

| | During Rest. 1st Exper. | 2d Exper. | During Motion. 3d Exper. |
|---|---|---|---|
| | July 4. | July 5. | July 11. |
| Temp. of air, | 90° F. | 84° F. | 83° F. |
| Wet bulb thermometer, | 78° F. | 76° F. | 74° F. |
| Experiment commenced at | 11.45 A.M. | 12.10 P.M. | 11.50 A.M. |
| Temp. of water when drawn, | 73½° F. | 73½° F. | 75° F. |
| Temp. of water at the end of an hour on entering the bath, | 74° F. | 74° F. | 75½° F. |
| Temp. of water at the close of an hour on leaving the bath, | 76½° F. | 76½° F. | 78° F. |
| Heat imparted to the water, deducting normal error, | 2° F. | 2° F. | 2° F. |
| Volume of water in the bath, | 7½ cubic feet. | | |
| Volume of the body, | 3 " " | | |
| Weight of the body, | 180 lbs. | | |
| Height of the body, | 5 feet 5¼ inches. | | |

In the first and second experiments I laid perfectly still; the results therefore show the quantity of heat passing off from the surface of the body in a state of rest. This, as the table indicates, could warm seven and a half cubic feet of water two degrees in one hour. The volume of the body being three cubic feet, it follows that if we consider the specific heat of the body as about the same as that of water (which it probably is), enough heat is evolved in the course of one hour to warm the body itself about five degrees of Fahrenheit's scale. The converse of this may also be considered as true, viz., that after death, the air being at 73°, enough heat is lost in the course of an hour to cool the body five degrees, at least during the first hour. It is therefore a fact of considerable importance from a

medico-legal point of view, especially in estimating the time a body has been immersed in water after recent drowning when the temperature of the water is about 73°, as is the case with the Croton and other streams in summer.

In the third experiment one or other of the lower extremities was alternately kept in motion during thirty minutes of the hour in which the body was immersed in the bath. The movement consisted in extending and flexing the leg on the thigh at the rate of fifty flexions per minute, and being performed under the surface of water involved considerable muscular exertion. Notwithstanding this violent exertion, as the table shows, there was no increase in the amount of heat imparted to the water. The consequences flowing from this result are of great physiological importance, but we reserve their consideration until we have completed the history of our experiments. We therefore pass to the examination of

*The physiological effects of the cold bath on the body.*

*Experiment of July 4,—Rest—Temp. of Bath 74° F.*

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | Temp. before entering the bath. | Immed. after entering the bath. | After 1 hour in the bath & just before leaving it. | Immed. after leaving the bath. | One hour after leaving the bath. | Two hours after leaving the bath. |
| Temp. of the mouth, | 99° F. | 99° F. | 98° F. | 97° F. | 97° F. | — |
| " " armpit, | 96° F. | 97° F. | 95° F. | 92° F. | 96° F. | — |
| " " temple, | 96° F. | — | — | — | 94° F. | — |
| Rate of respiration, | 20 | 22 | 16 | 13 | 16 | 19 |
| Rate of pulse, | 74 | 73 | 65 | 54 | 60 | 72 |

*Note.*—A chill or shock was experienced on entering the bath, and the sensation of coolness remained while in the water. Skin was dry and hot for an hour and a half after coming out. Perspiration set in and skin became cool two hours and a half after coming out. Shortly after leaving the bath slept for 30 minutes.

*Experiment of July 5,—Rest—Temp. of Bath 74° F.*

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | Before entering the bath. | Immed. after entering the bath. | After 1 hour in the bath & just before leaving it. | Immed. after leaving it. | One hour after leaving bath. | Two hours after leaving bath. |
| Temp. of mouth, | 99° F. | 99° F. | 98° F. | 97° F. | 97° F. | 98° F. |
| " " armpit, | 98° F. | 97° F. | 94° F. | 94° F. | 97° F. | 97° F. |
| " " temple, | 97° F. | — | 95° F. | 95° F. | 96° F. | 96° F. |
| Rate of respiration, | 17 | 21 | 18 | 15 | 16 | 16 |
| Rate of pulse, | 78 | 66 | 64 | 55 | 56 | 60 |

*Note.*—Symptoms same as in experiment 1, but not as well marked; slept 30 minutes as in preceding.

If in the tables we compare column 1, representing the condition before entering the bath, with column 4, representing the

condition immediately after leaving it, we find that in both experiments the exposure for one hour to water at a temperature of about 74° F. lowered the temperature of the mouth two degrees, of the armpit four degrees, and of the temple two degrees. The rate of respiration is also diminished in one case two and in the other four movements, and that of the pulse twenty beats in one and twenty-three in the other. It is therefore evident that the effects of the long continued application of a degree of cold such as that employed, is to reduce the temperature of the body and the rate of respiration slightly, while it affects the rate of pulsation in a very profound manner.

One of the consequences of this effect of cold on the action of the heart was a great reduction in the quantity of oxygen introduced into the system. The rate of pulsation being reduced nearly one-third, the quantity of oxygen conveyed into the interior of the body was diminished in a somewhat similar ratio. In a short time this began to exert its influence on the nervous centers, and there was a overwhelming disposition to fall asleep, which was unconsciously indulged in in both experiments shortly after leaving the bath, notwithstanding the strong desire to keep awake for the purpose of recording the rates of pulse and respiration at given periods.

Another evident consequence of such a sluggish movement of the blood is the disposition to congestion of various internal organs, and herein we may see a partial explanation of the action of cold in causing inflammations, especially of those organs engaged in the processes of secretion and excretion.

The discussion of the results obtained has thus far been confined to the consideration of columns 1 and 4. I have followed this course because, while in the bath, a slight access of water to the armpit or to the temple causes irregularities in the thermometric indications. In the case of the respiratory movements it is also very difficult to avoid influencing them in the act of counting. The mouth temperatures are, it is true, free from the influence of external agents, but the differences are too small to be perfectly reliable. In the case of the pulse determinations none of these objections can be urged; they are considerable, and by counting for half a minute for every record made, the error is reduced to a maximum of one beat. The movements of the heart are, in addition, free from the liability to error that exists in the case of the respiratory movements.

Accepting the pulse determinations as being accurate and reliable indications of the effects produced, both while in the bath and out of it, we may return to the consideration of the tables, and compare together columns 1, 3 and 4. Recollecting that 1 represents the condition on entering the bath, and 3 that just

before leaving it, after an immersion of one hour we find that the pulse was reduced nine beats in the first experiment, and fourteen in the second. If now we compare 3, the condition just before leaving the bath, with 4, the condition just after leaving it, we find that the rate of the pulse has diminished eleven beats in the first and nine in the second experiment. The explanation of this extraordinary reduction of the pulse rate on leaving the bath is by no means clear. One thing is, however, very evident, and that is, the profound effect of the application of cold, as is shown not only by the singular phenomenon of which we have just spoken, but also by the slowness with which the original rates of pulsation are regained, as is demonstrated by columns 5 and 6 of the tables.

The motion experiment of July 11th gave the same general physiological results as the rest experiments of July 4th and 5th. The difference being, that during the motion the respiratory movements became 30 per minute and the pulse 90, both regaining the rate represented in the rest experiments very soon after the cessation of the exercise. Placing this great increase of the respiratory movement in juxtaposition with the failure of the exercise to cause any perceptible increase in the temperature of the bath, it is evident that the contact of the cold water must put an almost absolute stop to the functions of the skin, and the whole duty of exhalation of vapor of water and consequent removal of heat is thrown on the lungs; hence the increased respiratory action, and hence also the special tendency of application of cold to the surface to produce inflammations of those organs by increasing the work they are obliged to perform, and raising the pulse-respiration ratio to that actually existing in pneumonia.

In conclusion, it may be observed that the primary and most important effect of the application of cold to the whole surface of the body is to reduce the action of the heart. This reduction is still further increased on removing the cold, if the application has continued for a sufficient length of time; and, as a consequence of the reduction of the heart's action, the phenomenon of stupor or sleep appears, produced either by deficient oxidation or by imperfect removal of carbonic acid. There is also a tendency to congestion of various internal organs, especially of the lungs, and the establishment of a pulse-respiration ratio similar to that of pneumonia.

College of the City of New York, Oct., 1872.

ART. LIV.—*On the Quartzite, Limestone and associated rocks of the vicinity of Great Barrington, Berkshire Co., Mass.;* by JAMES D. DANA.

(Continued from page 370.)

3. *Stratification.*

(A.) *Monument Mountain, and the Housatonic Valley adjoining it on the west.*

Monument Mountain has a precipice of hard quartzite on its eastern front,* and a wall of the same rock along the sides facing southwest and west, while through the interior the rock is mica schist and gneiss.

The hard quartzite of the walls is without bedding, but is jointed in various directions. The most regular and profound of these joints have approximately a north-and-south direction (N. 10° E. to N. 10° W.),† and are nearly vertical. Another system of divisional planes in the eastern mass of quartzite is strikingly apparent in a view from the Stockbridge road along the eastern foot (see map); the planes through the northern half dip northward 15° to 35°, increasing to the northward; and in the southern, southward 10° to 20°, the northern bending over into the southern. The former are also well seen along the mountain path near the *western* foot of this crest of quartzite, but the dip here in view is 45° to 50°. The strike on this western side is nearly east-and-west; at one place it was N. 70° E. The divisional planes of this system look as if due to bedding, and an anticlinal fold; and yet they are simply joints,—perhaps a result of contraction on cooling after consolidation.

The accompanying figures, 1 and 2, give a general idea of the stratification. The first is a section drawn through the mountain near its southwestern side.

1.

Section across Housatonic Valley and Monument Mountain.

* This eastern quartzite crest or ridge of Monument Mountain is the part usually ascended by tourists. A path leaves the Stockbridge road near *p* (see map) and passes up the interior of the mountain, near the western foot of this ridge, to its north end, where is the best place of ascent; a branch path leads off also to its south end. The view takes in the Catskills on the west, Graylock to the north, Mount Everett and the Taconic range to the southwest, and, nearer by, the beautiful valley of the Housatonic, with its lakes and villages.

† The courses given in this paper are compass courses. The variation in the Barrington region is about 8¼° W., which would make N. 10° E. compass course correspond to N. 1¼° E. true course.

H is the position of the Housatonic river, and a little south of the line of the section is the village of Housatonic. Near $q^2$ (*p* on the map) passes the road, first northeastward, then northward, to Stockbridge. As shown, there are in Monument Mountain two strata of quartzite, $q^1$, $q^2$, with two of schist, (mica schist and gneiss) $s^1$, $s^2$, all dipping southeasterly, the average slope 25°.

The thickness of the upper quartzite of Monument Mountain is 200 or 250 feet; that of the schist under it at least 500 feet; and that of the lower quartzite about 250 feet.

The schist ($s^2$) over the interior of Monument Mountain varies much in strike and dip, as is common in beds that are little inclined. The dip is for the most part to the east of southeast 20° to 25°, the strike being about N. 25° E. Just west of the eastern quartzite the amount of dip varied from 15° to 25°, and the direction was generally that just stated: yet in some places the strike was N. 50° W. and N. 70° W. At the quarry (*s* on the map), where there is a large quartz vein containing some black tourmalines (and a trace of copper pyrites) the bedding was much obscured by joints, but where best exhibited the strike was N. 25° E., and the dip 45° to 60°—an exception in its large amount to the general dip in the mountain.

There is no good outcrop of the lower schist ($s^1$) at the west end of Monument Mountain *east* of the Housatonic river; but the smooth surface of the foot hills or slopes in that part, and the sudden transition from numberless quartzite fragments to occasional masses of gneiss which is found on the descent of this side of the mountain, are evidence of its existence. Besides this, the stratum of schist is well exhibited on the opposite or *west* side of the river, where it first dips eastward at an angle of 20°, and then, farther from the river, *westward* 10° to 25°, the strike nearly north; a change of direction which indicates the existence along the valley of a low anticlinal, as represented in the section (fig. 1). A little farther west there is a bold ridge of quartzite, which is evidently the western side of the fold of the quartzite ($q^1$). A narrow depression or valley intervenes between the schist and this quartzite, so that the actual superposition of the former was not visible; but the dip of the schist was not only toward the quartzite on the east (as shown in the section), but also on the west of it in the Williamsville valley; and hence the quartzite is the course of a shallow synclinal. The quartzite was of the hard jointed kind, indistinct in its bedding. At one place I observed a westward dip of 20°—the strike N. 20° W.—in divisional planes which appeared to be those of the bedding.

The following section of Monument Mountain (fig. 2) represents the stratification along a line half a mile north of that of the first (fig. 1).

2.

Section across Housatonic Valley and Monument Mountain.

On the *west*, it passes the Housatonic river, near an old dismantled iron furnace, (*f*, on map), about three-fourths of a mile north of Housatonic village. On the *east*, it comes out in front of a limestone quarry, just west of the road to Stockbridge.

The upper quartzite, $q^2$, forms a bold precipice of 200 feet along the eastern front of the mountain. Below this and its talus, the slope is made up of great blocks of gneiss, many like houses in size, but all displaced, owing evidently to the wedging action of growing trees. This line of junction between the schist and quartzite, along the front, rises going *northward;* and at the northern limit of the quartzite crest, where this crest loses rather abruptly 150 feet or more of its height, it meets the stratified schist which is there in place as the top rock of the mountain; while *southward*, the upper line of the schist falls gradually in height, corresponding with the southeasterly dip of the strata. The schist of the interior of the mountain here passes through to the east face, beneath the bed of quartzite.

In addition to this evidence as to the true stratification and position of the quartzite, there is *distinctly stratified quartzite conformable beneath the gneiss*, at the west end of the mountain, toward the summit. The locality where I observed this bedded quartzite is just above the level of the hard bedless quartzite, and about a hundred yards to the east of it, at a point marked *t* in the map;* a weathered bluff of it much resembles in its regular lines of nearly horizontal stratification the bluff of gneiss that forms the western brow of the mountain a hundred feet above it. This bedded quartzite is a somewhat crumbling rock, and the decomposition of the lower layer is undermining the bluff. It lies conformably beneath the schist; the dip of the quartzite is 15° to 20°, the strike N. 25° E., and the dip of the schist above and a little farther east 25°, with the same strike. The depression on the east side of the hard western quartzite (indicated in both sections), varying in depth from 40

* The point *t* may be reached by taking a quarry road at *r* (see map) and following it beyond the quarries (*s*) until the road becomes a mere path (at *t*). The bluff of bedded quartzite is to the right of the path, a little distance from it.

to 80 feet, is probably due to the abrupt change in the rock from the hard jointed to the softer bedded kind.

Traces of similar bedding, and of like softness in the rock where bedded, occur at the northern margin of the *eastern* crest of quartzite, along the path descending northward; also in some places near the hard quartzite of the southwestern wall, where, at places, I passed isolated masses of great size undergoing deep disintegration, that bore evidences of the great amount of degradation which had taken place around them.

Descending the western slope, toward the Housatonic river and village, the quartzite is passed; and then a region of schist, indicated (as stated above) by a sudden substitution over the slopes of loose masses of schist in place of quartzite; and, finally, within less than a hundred feet of the base, on the path leading northwestward toward the old furnace, there is an outcrop of *limestone* with a slight easterly dip. West of the river the same limestone is exposed in the slopes beyond the road, and it there has a *westerly* dip (10° to 25°, the strike north) as shown in the section; and this limestone, at a point just west of a quarry, is visibly and conformably overlaid by mica schist, dipping 25° to the westward. Neither the limestone of section 2 nor the lower schist ($s^1$) (mica schist and gneiss, one passing into the other) of section 1 is exposed to view in the river banks, on account of the deep covering of stratified drift, so that the thickness of the schist was not ascertained; it probably does not exceed 50 feet. The limestone is the true Stockbridge limestone.

The same low anticlinal is here apparent that is represented in figure 1; and it is further evident that the anticlinal has an inclined axis dipping southward, inasmuch as the limestone, an inferior stratum in the fold, is exposed on going north, while covered to the south.

The mica schist of the interior of Monument Mountain, and part of the gneiss layers, decompose rapidly and deeply, and by this means much of the region is deeply covered with earth, which is partly clayey.

The main propositions stated at the commencement of this paper on the Great Barrington region—that the quartzite occurs interstratified conformably with the Stockbridge limestone and the accompanying mica schist and gneiss, and that the quartzite is one of the newer rocks in this part of the Green Mountains—appears to be well established by the facts already adduced. But others no less decisive are afforded by the country adjoining on the west, south, and east.

[To be continued.]

ART. LV.—*On the relation between Color and Geographical Distribution in Birds, as exhibited in Melanism and Hyperchromism;* by ROBERT RIDGWAY.

THE two chief modifications of color experienced in the several geographical, or climatic, regions of the North American continent, by certain species of birds which are resident over a very extended area, are the following:—I. *A melanistic tendency*, which may be either an increase in the intensity of color or in the extent, of the black parts of the plumage; and II. *A greater brightness, or an increased prevalence, of the three primary colors, red, blue, and yellow.*

These features are mainly noticeable as the result of a tropical influence, for they are most highly developed in middle America, and become exaggerated in proportion to the decrease of latitude. But in the Pacific province of North America they are, in many cases, either entirely similar, or represented by somewhat modified analogous laws: thus, the law of melanism seems to be equally, if not more specially, characteristic of the Pacific province; the degree of modification of yellow tints also appears to be about the same in the two regions; but in regard to red, the rule seems to be that in middle America it is increased in *intensity*, and in the Pacific province of North America it is increased in extent, or *amount;* while blue seems to be affected latitudinally only, its prevalence, in certain cases, increasing as we trace a species southward.

These generalizations may be best illustrated by presenting the following especially noteworthy cases:

I. *Melanism.* A striking example in illustration of this law is found in *Chrysomitris psaltria*, under which we range as races, *C. Arizonæ* Coues, *C. Mexicana* Swains., and *C. Columbiana* Lafr. Specimens of this bird from the southern portion of the Western province of the United States (Rocky Mountains to California, its northern limit being about the parallel of 40°), have the black of the upper parts confined to the head, wings and tail, the entire dorsal region being olive-green; this form constitutes the true, or restricted, *C. psaltria.** Examples from Arizona, New Mexico, and the northern provinces of Mexico (var. *Arizonæ*),† have this olive-green clouded, or mixed, with

* CHRYSOMITRIS PSALTRIA var. PSALTRIA.
*Fringilla psaltria* Say, Long's Exp., ii, 1823, 40.—*Chrysomitris psaltria* Bonap. List, 1838.—Baird, B. N. Am., 1858, 422.
*Hab.* Rocky Mts. and Middle province of U. S., north to about 40°.

† CHRYSOMITRIS PSALTRIA var. ARIZONÆ.
*Chrysomitris Mexicana* var. *Arizonæ* Coues, P. A. N. S., Philad., 1866.—Cooper, Orn. Cal., i, 1870, 170.
*Hab.* Southern border of U. S., in New Mexico and Arizona.

black, there being more of the latter color in the Mexican than in the Arizonan specimens. By the time we reach the latitude of Mirador and Mazatlan, the black entirely replaces the olive-green; the bird now is the var. *Mexicana*,* and continues with nearly the same characteristics south to Costa Rica and Panama, from which latter countries we find specimens in which the black is often appreciably more intense and lustrous than in those from Mexico. These three forms all have white on the tail; but in specimens from New Granada, and occasionally in those from Panama, there is usually a total absence of the white marks on the tail, or else they are greatly reduced in size. These equatoreal specimens (var. *Columbiana*)† exhibit the extreme of melanism in this species; and when compared with specimens of var. *psaltria*, only or without any of the intermediate races, appear so different as to convey the impression of entire distinctness; but when we bring together a large series, such as that contained in the Smithsonian collection, and notice how the increasing proportion of black strictly coincides with the decreasing latitude of the localities of the specimens, and observe, too, that it is utterly impossible to draw a latitudinal boundary to separate any two of these several races, the conclusion that they all represent climatic modifications of one species seems unavoidable. The females, it is well known, can scarcely be distinguished by experts.

*Myiarchus Lawrencii*, which, starting with a grayish brown crown in the most northern examples (var. *Lawrencii*),‡ gradually assumes a blacker and blacker crown, as we trace it southward, first through var. *nigricapillus*§ (Costa Rica and Panama), and finally ending in var. *nigriceps*‖ (Ecuador), which has the

* CHRYSOMITRIS PSALTRIA var. MEXICANA.
*Carduelis Mexicana* Swains., Syn. Birds Mex., Phil. Mag., 1827, 435.—*Chrysomitris M.* Bonap., Consp., 1850, 516.—Baird, B. N. Am., 1858, 423, pl. liv, f. 1.
*Hab.* Middle America (coast to coast), from Northern Mexico to Costa Rica.

† CHRYSOMITRIS PSALTRIA var. COLUMBIANA.
*Chrysomitris Columbiana* Lafr., Rev. Zool., 1843, 292.—Baird, B. N. Am., 1858, 423.—Sclater, Catal., 1862 124.
*Hab.* Bogota to Panama.

‡ MYIARCHUS LAWRENCII var. LAWRENCII.
*Tyrannula Lawrencii* Giraud, 16 sp. Texas Birds, 1841, pl. ii.—*Myiarchus L.* Baird, B. N. A., 1858, 18.—Sclater, Cat. Am. B., 1862, 233.—Coues, P. A. N. S., July, 1872.
*Hab.* Mexico, from near U. S. boundary southward to Mirador, Mazatlan, etc., and including both coasts.

§ MYIARCHUS LAWRENCII var. NIGRICAPILLUS.
*Myiarchus nigricapillus* "Cabanis, MS.," Sclater, Cat. Am. B., 1862, 233.—*M. Lawrencii* Coues, P. A. N. S., July, 1872.
*Hab.* Middle America, from Southern Mexico to Panama.

‖ MYIARCHUS LAWRENCII var. NIGRICEPS.
*Myiarchus nigriceps* Sclater, P. Z. S., 1860, pp. 68, 295 (Ecuador).—Ib., Catal. Am. B., 1862, 234.—Coues, P. A. N. S., July, 1872.
*Hab.* Panama to Ecuador.

crown deep black. *Sayornis nigricans*,* from California and Northern Mexico, has the crissum pure white; Mirador specimens have it clouded with dusky, while in Costa Rican specimens (var. *aquaticus*),† it is entirely blackish, only the middle of the abdomen being white. In all these cases the change is gradual, the two extremes being connected by a perfectly unbroken series of intermediate specimens. *Vireosylvia gilva* of the United States has the crown of an ashy gray color, and tropical American examples (var. *Josephæ*, Costa Rica and Ecuador), have it dark snuff-brown, while examples from Mirador, Mexico, are as exactly intermediate in colors as they are in habitat.

The same law as regards the Pacific province of North America is made evident by the well known cases of *Picus villosus* var. *Harrisii*,‡ *P. pubescens* var. *Gairdneri*,‖ *Sphyropicus varius* var. *ruber*,¶ the Northwest coast forms of *Falco peregrinus*, *F. Columbarius*, *Bubo Virginianus*, *Scops asio*, and numerous other similarly affected species.

II. *The law affecting the primary colors.*—Of this class we may begin with yellow, as being the color whose changes are most nearly parallel with those of black, i. e., affected nearly similarly in middle America and the Pacific province of North America. The following cases afford illustrations:—*Xanthoura luxuosa*** from the Rio Grande of Texas and Northern Mexico, has the lower parts deep green, while the same species from Guatemala (var. *Guatemalensis*),†† is pure gamboge yellow

* SAYORNIS NIGRICANS.
*Tyrannula nigricans* Swains., Phil. Mag., 1827, i, 367.—*Sayornis n.* Bonap., Compt. Rend., xxxviii, 657.—Baird, B. N. A., 1858, 183.; Scl., Cat., 1862, 200.
*Hab.* California and Northern Mexico.

† SAYORNIS NIGRICANS var. AQUATICUS.
*Sayornis aquaticus* Scl. and Salv., Ibis, 1859, 119 (Guatemala).
*Hab.* Central America, from Southern Mexico to Panama.

‡ PICUS VILLOSUS var. HARRISII.
*Picus Harrisii* Aud., Orn. Biog., v, 1839, 191, pl. 417.—Baird, B. N. Am., 1858, 87.
*Hab.* Western Province of N. Am., and south into Mexico, where it grades into the smaller and still darker var. *Jardini*, which reaches its extreme condition in Costa Rica.

‖ PICUS PUBESCENS var. GAIRDNERI.
*Picus Gairdneri* Aud., Orn. Biog., v, 1839, 317.—Baird., B. N. Am., 1858, 91.
*Hab.* Western Province of N. Am.

¶ SPHYROPICUS VARIUS var. RUBER.
*Picus ruber* Gmel., S. N., i, 1788, 429.—*Sphyropicus ruber* Baird, B. N. Am., 1858, 104.
*Hab.* Pacific Province of N. Am.

** XANTHOURA LUXUOSA var. LUXUOSA.
*Garrulus luxuosus* Lees., R. Z., April, 1839, 100. *Xanthoura l.* Bonap., Consp., 1850, 380.—Baird, B. N. Am., 1858, 589.
*Hab.* Mexico, from the Rio Grande to Isth. Tehuantepec.

†† XANTHOURA LUXUOSA var. GUATEMALENSIS.
*Xanthoura guatemalensis* Bonap., Consp., p. 380.
*Hab.* Yucatan, Guatemala and adjacent portions.

beneath, intervening localities producing specimens having a mixture of green and yellow below—either color predominating, according to the proximity to the region of the extreme to which it tends. Specimens of *Myiodioctes pusillus** from the Pacific coast of North America (var. *pileolata*,† Alaska to Costa Rica), are decidedly richer yellow than eastern ones; and the olive-green above inclines more to yellow. Specimens of *Helminthophaga celata* from the same region (var. *lutescens*),‡ are altogether more yellowish than eastern ones. The case of the genus *Geothlypis*§ furnishes another striking example of the tropical influence upon this color, and also affords a series of facts which lead to generalizations of other kinds.

The following synopsis—the result of the joint investigations by Professor Baird and myself, and modified from that already in print in our forthcoming work upon North American Birds, chiefly by the introduction of *G. speciosa* and *G. semiflava*, the types of which, being in London, we have not seen, and which, until Mr. Salvin's notice of them, had not been sufficiently well characterized—will, perhaps, render the remarks which follow it more lucid:

*Synopsis of the species and their subordinate races of the genus Geothlypis.*

**Throat yellow, Series I; Throat ashy, Series II.**

**Series I.**

**A. Black "mask" extending backward beneath the eye, on to the auriculars. Bill slender, the culmen nearly straight (as in *Oporornis*).**

**1. G. trichas. Crown whitish; maxillæ black.**

**Abdomen almost always whitish; occiput russet-olive. Bill, from nostril, .30; tarsus, .70; wing, 2.25; tail, 2.15. *Hab.* Whole of the United States in summer; in**

* Myiodioctes pusillus var. pusillus.
*Muscicapa pusilla* Wils., Am. Orn., iii, 103, pl. 26, fig. 4.—*Myiodioctes pusillus* Bp., Consp., 815. Baird, B. N. Am., 1858, 293 (in part).
*Hab.* Eastern Province and Rocky Mts. of N. Am.; eastern middle America in winter.

† Myiodioctes pusillus var. pileolata.
*Motacilla pileolata* Pallas, Z. R. As., i, 1831, 497.
*Myiodioctes pusillus* var. *pileolata* Ridgway.

‡ Helminthophaga celata var. lutescens.
*Helminthophaga celata* var. *lutescens* Ridgway.
*Hab.* Pacific Coast, from Radiak to Cape St. Lucas.

§ A very interesting paper upon the genus *Geothlypis*, by Mr. Salvin, appears in the London *Ibis*, for April, 1871. In this valuable article several generalizations are made, and theories suggested, which we may be able to corroborate by a few additional facts bearing upon the relationship of the several species of this genus and their subordinate forms.

winter south through middle America to Chiriqui, and throughout most of the West India Islands.

*α. trichas.**

Abdomen yellow; occiput russet-olive. Bill, ·45; tarsus, ·90; wing, 2·50; tail, 2·50. *Hab.* Nassau, Island of New Providence, Bahamas; resident.

*β. rostrata.*†

Abdomen bright yellow; occiput whitish ash, tinged with yellow. Bill, ·32; tarsus, ·75; wing, 2·45; tail, 2·50. *Hab.* Eastern Mexico (Xalapa?). *γ. melanops.*‡

2. G. SPECIOSA. Crown black; abdomen ochraceous; bill wholly black. Wing, 2·40; tail, 2·30. *Hab.* Eastern Mexico. *α. speciosa.*§

Abdomen bright yellow; bill with the lower mandible yellowish. *Hab.* Ecuador. *β. semiflava.*‖

3. G. ÆQUINOCTIALIS. Crown deep ash; maxillæ yellow.

Black of the auriculars bordered posteriorly by the olive-green of the nape. Bill, ·17 deep; wing, 2·50; tail, 2·35. *Hab.* Northeastern South America (Trinidad, Guiana, Venezuela and New Granada).

*α. æquinoctialis.*¶

Black of the auriculars bordered posteriorly by the ash of the crown.

Forehead narrowly black. Bill, ·14 deep; wing, 2·40; tail, 2·50. *Hab.* Southern South America (Brazil, Paraguay, Buenos Ayres, etc.). *β. velata.***

Forehead broadly black. *Hab.* Chiriqui.

*γ. Chiriquensis.*††

B. Black mask not extending beneath the eye, but confined to the lores and a narrow frontlet. Bill thick, the culmen curved (much as in *Granatellus*).

* GEOTHLYPIS TRICHAS var. TRICHAS.
*Turdus trichas* Linn., S. N., 1766, 293.—*Geothlypis t.* Caban., Mus. Hein., 1860, 16.—Baird, B. N. Am., 1858, 241; Rev. Am. B., 1864, 220.

† GEOTHLYPIS TRICHAS var. ROSTRATA.
*Geothlypis rostratus* Bryant, Pr. Bost. Soc., Oct., 1866, 67.

‡ GEOTHLYPIS TRICHAS var. MELANOPS.
*Geothlypis melanops* Baird, Rev. Am. B., 1865, 222.

§ GEOTHLYPIS SPECIOSA var. SPECIOSA.
*Geothlypis speciosa* Scl., P. Z. S., 1858, 447. (We are unable to describe these two races more exactly, since we have not been able to see specimens; while the published descriptions are lacking in sufficient details.)

‖ GEOTHLYPIS SPECIOSA var. SEMIFLAVA.
*Geothlypis semiflavus* Scl., P. Z. S., 1860, 273, 291.

¶ GEOTHLYPIS ÆQUINOCTIALIS var. ÆQUINOCTIALIS.
*Slotacilla æquinoctialis* Gm., S. N., 1788, 972.—*Geothlypis æq.* Caban., Mus. Hein., i, 1860, 16.—Baird, Rev. Am. B., i, 1865, 224.

** GEOTHLYPIS ÆQUINOCTIALIS var. VELATA.
*Sylvia velata* Vieill., Ois. Am. Sept., ii, 1807, 22, pl. lxxiv.—*Geothlypis velatus* Caban., Mus. Hein., i, 1850, 16.—Baird, Rev. Am. B., i, 1865, 223.

†† GEOTHLYPIS ÆQUINOCTIALIS var. CHIRIQUIENSIS.
*Geothlypis chiriquiensis* Salvin, Ibis, April, 1872. (No measurements are given in Mr. Salvin's description.)

4. G. POLIOCEPHALA. Crown wholly ash; maxillæ yellow. Eyelids white; nape and auriculars olive-green; abdomen whitish. Bill, ·30–·15 deep; wing, 2·20; tail, 2·50. *Hab.* Western Mexico (Mazatlan).

*α. poliocephala.**

Eyelids black; nape and auriculars ashy; abdomen wholly yellow. Bill, ·35, ·18 deep; wing, 2·40; tail, 2·50. *Hab.* Guatemala, and Costa Rica. *β. caninucha.*†

SERIES II.

5. G. PHILADELPHIA. Head and neck wholly ashy. Eyelids dusky; lores dusky, not in strong contrast with the ash; black centers of the feathers of the gular region larger, or expanded, posteriorly, suffused with a patch—sometimes uniform—on the jugulum. Tail, 2·05 to 2·15. *Hab.* Eastern province of North America, south in winter (migrating across the Gulf of Mexico and Caribbean Sea, without stopping by the way!) to Costa Rica, Panama and Bogota.

*α. Philadelphia.*‡

Eyelids pure white; lores deep black in strong contrast with the ash; black centers of the feathers of the gular and jugular region, not larger posteriorly, and showing no disposition to form a patch on the jugulum. Tail 2·25 to 2·50. (Female distinguishable only by longer tail, more distinctly white eyelids, and more dusky lores.) *Hab.* Western province of North America, from British Columbia to Costa Rica. *β. Macgillivrayi.*§

In studying closely the affinities of the different forms given in the above synopsis, one of the most striking facts noticeable is that all of the peculiarly southern species, except *G. poliocephala*, have the belly wholly yellow; while the most northern species (*G. trichas*), with the belly whitish in the northern form, has it yellow in the two southern ones; two of the tropical American species have also the belly whitish in their northern (*G. poliocephala* and *G. speciosa*) and yellow in their southern races (*G. poliocephala*, var. *coninucha*, and *G. speciosa* var. *semiflava*). These facts we consider as evidences, if not proofs, of a tropical

* GEOTHLYPIS POLIOCEPHALA var. POLIOCEPHALA.
*Geothlypis poliocephala* Baird, Rev. Am. B., i, 1865, 225.

† GEOTHLYPIS POLIOCEPHALA var. CANINUCHA.
*Geothlypis poliocephala* var. *caninucha* Ridgway.

‡ GEOTHLYPIS PHILADELPHIA var. PHILADELPHIA.
*Sylvia Philadelphia* Wilson, Am. Orn., ii, 1810, 101, pl. xiv.—*Geothlypis Philad.* Baird, B. N. Am., 1858, 226; Rev. Am. B., 1865, 226.

§ GEOTHLYPIS PHILADELPHIA var. MACGILLIVRAYI.
*Sylvia Macgillivrayi* Aud., Orn. Biog., v, 1839, 75, pl. 399.—*Geothlypis Mac.* Baird, B. N. Am., 1858, 224, pl. lxxix; Rev. Am. B., i, 1865, 227.

influence, whereby yellow is extended or intensified in intertropical regions. In the North American series of *G. trichas* (of which there are over one hundred and fifty specimens in the Smithsonian collection), it is also noticed that while specimens from the Atlantic States have, almost universally, whitish bellies, those from the valley of the Mississippi (as far north as Wisconsin), where the climate is much more warm and humid than that of the Atlantic coast in corresponding latitudes, as well as those from the Gulf States, frequently have the belly wholly yellow, and closely approach in characters also the Mexican race, var. *melanops*. Middle American examples of var. *trichas* are precisely like those from the United States, they being merely winter visitants to the south from the latter country. The var. *rostrata* we can only consider as a gigantic insular race of the common species, its yellow belly being due to its permanently southern habitat, and its larger size to be accounted for by local causes, as yet unknown. Specimens of var. *trichas* from the Pacific coast differ from eastern ones only in longer tails,* and in having a more yellowish tinge in the olive-green of the upper parts; so also has the western race of *G. poliocephala* a longer tail, and more yellowish cast to the upper plumage, than its eastern form (var. *coninucha*). While the western form of this species has a longer tail than the eastern one, and white, instead of dusky, eyelids, so also has that of *G. Philadelphia* (var. *Macgillivrayi*). In the case of *Myiarchus Lawrencii*, before alluded to, it is noticed that the yellow of the abdomen increases in richness, just at the same rate that the blackish of the pileum does in intensity, as it approaches its most southern extreme.

[To be continued.]

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ART. LVI.—*A Theory of the Formation of the great Features of the Earth's Surface;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist. University of California.

[Concluded.]

As already stated, every other theory fails to account for the immense crushing together shown by *plication* and *slaty cleavage*. Many theories take cognizance of this crushing, but in all it is a *subordinate accompaniment* instead of the *cause* of the elevation. Let us examine very briefly some of the more recent theories, and show their inadequacy.

* For a discussion of this law see Baird, in this Journal, vol. xli, March, 1866, pp. 21, 22.

Prof. Hall undoubtedly deserves the thanks of geologists for first strongly drawing attention to the fact that mountain chains consist essentially of immense masses of sediments, much thicker, indeed, than the height of the mountains themselves. His views on this subject form, I believe, an era in the history of geological science. Nevertheless, I think his theory entirely fails to explain the actual process by which mountain chains have been formed, and especially to account for the immense horizontal crushing and plication of the strata. According to Hall's view, as explained by himself and by Sterry Hunt,* the Appalachian chain has been formed as follows: This chain consists of sediments 40,000 feet thick, which thin out as we go west, until at the Mississippi river they are only 4,000 feet. We may regard the whole, therefore, as originally an immense *convex* mass of submarine sediment. By *slow subsidence* of this *convex* mass the upper strata were subjected to horizontal squeezing and thereby thrown into folds. *Continental* upheaval exposed the *still somewhat convex* mass of plicated strata *as a great plateau.* Subsequent erosion formed the ranges and ridges with their intervening valleys. Thus the Appalachian chain, and, in fact, mountain chains generally, become mere fragments of denuded plateaus upheaved by continental elevation only. This, I think, is a brief but fair exposition of the theory.

Now, as Whitney† and Hunt, and Billings, and others have shown, when we recollect the breadth of the Appalachians (at least 100 miles), and therefore the gentleness of the supposed convex curve, the amount of crushing together by subsidence would be inadequate to account for the immense plication. But the degree of inadequacy is, I think, scarcely appreciated. We have already said that slaty cleavage shows in many cases a crushing of $2\frac{1}{2}$ miles into 1 mile. To produce such crushing by subsidence alone, the height of the convex mass would have to be greater than its base. Or even making every allowance for the fact that the area of principal plication is only 20 or 30 miles wide, still the height necessary would be enormous.

Besides, it is certain that the sedimentation was not finished first, and then afterward the subsidence occurred, but these two phenomena went on together *pari passu*; and, therefore, the surface was never convex at all, but nearly or quite horizontal all the time. Subsidence under such circumstances might produce horizontal *tension* or stretching of the lower strata, but could not produce horizontal *crushing* and plication of the upper strata.

* "Some points in American Geology," this Jour., May, 1861.

† Whitney, Mountain Building, p. 101. Hunt, American Geology, this Jour., May, 1861.

In addition to this, after the whole of the Appalachian sediments had been deposited, at the end of the Paleozoic era and immediately before these mountains were formed, the Appalachian region was nearly or quite on a level with the sea, being, in fact, during the Coal period, alternately a coal marsh and an estuary, and therefore *lower than the regions east and west of itself.* The mountain formation was a process entirely distinct from and subsequent to the sedimentation and the subsidence. The whole process seems to have been, first, an immense sedimentation and subsidence going on *pari passu* during the whole Paleozoic era; then, at the end of that era, a horizontal crushing together and folding of the strata, and an upswelling of the whole mass. Hall and Hunt leave the sediments just after the whole preparation has been made, but before the actual mountain formation has taken place; and, therefore, in the language of Dana, "it is a theory of mountains with the mountains left out."

Whitney,* in his admirable essay on Mountain Building, if I understand him aright, thinks plication the result of the subsidence of a mountain axis, previously elevated by other agency. The subsidence of such an elevated axis would indeed produce powerful horizontal thrust, and might therefore produce some plication. But why suppose a *previous* elevation at all, when the horizontal thrust necessary, by his own view, to produce the elevation, would itself produce the plication? Or how high must have been the axis to have produced by subsidence such plication as we often find! Or how was this enormously elevated mass supported? It is evident that the objection to Prof. Whitney's view is precisely the same as to Prof. Hall's.

Mountain chains and mountain ranges are therefore, I think, beyond question, produced by horizontal thrust crushing together the whole rock mass, and swelling it up vertically; the horizontal thrust being the necessary result of secular contraction of the interior of the earth. The smaller inequalities, such as ridges, peaks, gorges, and, in fact, nearly all that constitutes scenery, are produced by subsequent erosion.

I feel considerable confidence in the substantial truth of the foregoing statement of the mode of formation of *mountain chains.* As to the mode of formation of *continents* and *sea bottoms*, I feel less confidence. It is possible that even these may be formed by a similar unequal yielding to horizontal thrust, and a similar crushing together and up-swelling. If so, it would be necessary to suppose the amount of horizontal yielding in this case much less, but the depth effected much greater, than in the case of mountain chains. But, as we find no un-

* Mountain Building, &c., p. 106.

mistakable structural evidence of such crushing, except in the case of mountain chains, I have preferred to attribute the formation of continents and sea bottoms to unequal *radial* contraction.

I wish next to show that this theory of mountain chains *explains* in a satisfactory manner not only the mountain elevation and the phenomenon of plication and slaty cleavage, but also *all the most conspicuous phenomena of mountain chains and of igneous agencies.* The satisfactory explanation of these become, of course, strong evidence of the truth of the theory. The further development of the theory will be best undertaken in connection with the explanation of these phenomena.

(A.) *Thick sediments of mountain chains.* It is a well-known fact, first brought prominently forward by Prof. Hall, that mountain chains are composed of enormous masses of sediments. This fact forms the basis of Hall's sedimentary theory. Prof. Whitney,* it is true, thinks that the sedimentary theorists have mistaken cause for effect,—that thick sediments are not the cause of mountains, but mountain chains are the cause of thick sediments. He believes that a granite axis upheaved out of the sea has furnished by erosion the sediments which have been deposited on their flanks. But when we remember the immense thickness of these sediments and their extent, and the comparative narrowness of the granite axis which furnished their materials, we may well ask what must have been the original altitude of this granite axis! It seems impossible that the granite axis of a chain should have furnished by its erosion the immense mass of sediments involved in the structure of the whole chain. Not only so, but in many chains the strata are not only found on the flanks, but even the highest peaks are stratified. And not only so, but many chains, like the Appalachians and the Jura, have no granite axis at all from which to obtain their sediments. Whitney regards these latter as exceptions, and as always comparatively small chains, and probably formed in a different manner from the great chains with granite axes. My own belief is that all, smaller and greater, have been formed in a similar manner. Mountain sediments, I believe, are not the débris of the *granite axis* of the chain; they are evidently the débris of *continental erosion. Mountain chains*, strata, granite axis and all, *are off-shore deposits.* To state the proposition more definitely: *Mountain chains are formed by the mashing together and the up-swelling of sea bottoms where immense thickness of sediments have accumulated; and as the greatest accumulations usually take place off the shores of continents, mountains are usually formed by the up-pressing of marginal sea bottoms.* We will make this plainer by some illus-

* Mountain Building, &c., pp. 102 and 103.

trations taken from the history of mountain chains in North America.

*Appalachians.*—The area now occupied by the Appalachian chain was, during the Silurian and Devonian ages, the *eastern margin of the bed of the great interior Paleozoic sea.* During all this time the whole Paleozoic sea, but especially this *eastern margin*, received sediments from a continental mass to the northward (the Laurentian area), and also especially from a *continental mass to the eastward.* Besides the marks of shore deposit found abundantly in the Appalachian strata, other evidences are daily accumulating that the area to the east of the Appalachian chain, left blank in the geological map of the United States in Dana's text book—the so-called primary or gneissic region of the Atlantic slope—is Laurentian, and therefore was probably *land* during the Paleozoic times. The size of this eastern continental mass it is impossible for us now to know, as it has been partly covered by later deposits, and perhaps even partly covered by the sea; but, judging from the quantity of sediments carried into the Paleozoic sea, and especially from the thickness of the sediments (30,000 feet) along its eastern margin, derived probably wholly from this source, it must have been very large.

At the end of the Devonian age, much of the middle portion of the interior Paleozoic sea was upheaved and became land (see Dana's map, Manual, p. 133); and the Appalachian area now became alternately a coal marsh and an estuary emptying into the sea southward. Into this estuary or marsh, during the whole Coal period, sediments were brought from land north, east, and west, until 10,000 feet more had been deposited. During the whole of this time (Paleozoic era), while the 40,000 feet of sediments were depositing, this area—whether sea-margin bottom, or estuary bottom, or coal marsh—slowly subsided, so that nearly the same level was maintained. It was either shallow water or marsh all the time. This is certain for the Coal period, and almost equally certain for the previous periods. Moreover, it seems to be a general law throughout the whole geological history of the earth, that areas of great sedimentation have been also areas of subsidence *pari passu.* The same seems to be true now. Nearly all great river-deltas are slowly subsiding. In fact, in all shallow water deposits, and therefore in all shore deposits, the accumulation would soon cease, and therefore never become thick, but for the subsidence which constantly renews the conditions of deposit. The subsidence of the Appalachian area, therefore, must have been 40,000 feet vertical.

During the Coal period, therefore, the Appalachian region was still nearly on a level with the sea. So far from being a

convex plateau, it was a north-east and south-west trough. So far from being a mountain chain, it was evidently lower than the regions east and west of itself. At the end of this period occurred the Appalachian revolution. *The great mass of sediments which had been accumulating for so many ages, with their included seams of coal, yielded to the horizontal thrust,* was crushed together, and folded and swelled upward to a height proportionate to the horizontal crushing. Thus was the Appalachian formed—subsequent denudation has made it what it now is. It is probable that in the process of the up-pushing of the chain (or possibly at a later time) the eastern continental mass was diminished by subsidence.

*Sierras.*—We have good reason to believe that, at least some portion of the area now occupied by the Rocky Mountains was dry land even during the Paleozoic era. To what extent or what height we do not know. I shall say nothing of the formation of this the oldest portion of the North American Cordilleras, as the history of its formation is little known. I will commence with a considerable body of land which certainly existed in this region at the beginning of the Mesozoic era. Now, during the whole Triassic and Jurassic periods, *the region now occupied by the Sierras was a marginal sea bottom, receiving abundant sediment from a continental mass to the east.* At the end of the Jurassic, this line of enormously thick off-shore deposits yielded to the horizontal thrust, and the sediments were crushed together and swelled upward into the Sierra range. All the ridges, peaks, and cañons—all that constitutes the grand scenery of these mountains—has been the result of an almost inconceivable subsequent erosion.

*Coast range.*—The up-squeezing of the Sierra range, of course, transferred the coast line farther westward, and the region now occupied by the coast range became the marginal sea bottom. This in its turn received abundant sediments from the now greatly enlarged continent until the end of the Miocene, and then it also yielded in a similar manner and formed the coast range.

Thus I think it quite certain that the places now occupied by mountain chains have been always previous to their formation places of great sedimentary deposit, and therefore most usually marginal sea bottoms, since this is the most usual place for great deposits. In some cases, however, probably in many cases, *the deposits in interior seas or mediterraneans have yielded in a similar manner, giving rise to more irregular chains* or groups of mountains. This may have been the case with some of the more irregular mountains of Europe.

(B.) *Position of mountains along the margins of continents.*—The view that mountain chains are the up-squeezed sediments

of marginal sea bottoms completely explains the well-known law of continental form, viz., that continents consist of interior basins with *coast chain rims*. In fact, the theory necessitates this as a *general* form of continents, but at the same time prepares us for exceptions in cases of mountains formed from mediterranean sediments. The view is best illustrated from the American continent, because of the regular manner in which this continent has been developed. Nearly all geological problems seem to be reduced to their simplest terms, and therefore are most easily studied and understood in America.

Prof. Dana, in a paper on "the plan of development of the American continent,"* brings out some grand views on the relation of the heights of coast chains and their position, to the size and depth of the oceans which they overlook. From these formal laws, and proceeding on the hypothesis of a fluid interior, he concludes that sinking sea bottoms, determined by interior contraction, is the force by which continents are elevated. According to him, the sinking sea bottoms, together with the lateral thrust produced by interior contraction, push up the continents, at the same time crumpling up their margins into mountain chains. Such a process might certainly account for coast chains, for their position at right angles to the greatest expanse of ocean, and for their heights and crumplings being in proportion to the size and depth of the contiguous oceans; but the mechanics of the process is, it seems to me, untenable. For observe: this subsidence cannot be gravitative subsidence; for this could not raise continents. It is evidently a concave bending of the sub-oceanic earth-crust pressing on the liquid interior, and through it pushing up the continental crust. Now I have already shown that no stiffness of crust—not even if the crust were several hundred miles thick—could stand such strain over such immense areas. While I admire, therefore, the formal laws of Prof. Dana, I cannot accept his physical explanation.

(c.) *Parallel ranges.*—Whitney, in his essay on Mountain Building, already referred to, has drawn attention to the fact that the celebrated law of Elie de Beaumont, that parallel ranges of mountains are of the same age, so far from being true is nearly the opposite of the truth. Parallel ranges, at least of the same great system, are nearly always successively formed; and I would add *successively formed coastward.* He illustrates this by reference to the three great ranges of the North American Cordilleras, viz., the Rocky Mountains, the Sierras, and the Coast range—and by the several ranges forming the South American Andes. The theory I have presented at once explains this fact, and erects it into a law. It is a necessary result of the theory.

* This Jour., II, vol. xxii, p. 335.

In this connection, I will throw out a suggestion. Attention has been often directed to the truly wonderful submarine ridges and hollows brought to light by the U. S. Coast Survey, as occurring in the course of the Gulf stream, and extending all along the coast from the point of Florida to the coast of New England.* These ridges are truly submarine mountain ranges running parallel with the coast, and to the Appalachian.

7.

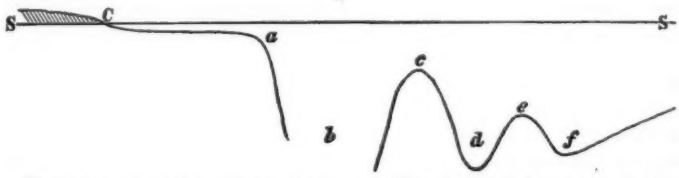



Fig. 7 is a rude section diagram illustrating the submarine configuration. Commencing at Charleston, C, the sea-bed slopes very gradually, so that at the distance of 50 miles (*a*) it attains the depth of only 20 fathoms. From this point it slopes very abruptly, so that it quickly attains unfathomable depth (*b*). At the distance of 100 miles from shore, at the depth of 300 fathoms, is found a ridge (*c*) rising from unfathomable depth on the coastward side, and 1,500 feet above the hollow (*d*) on the seaward side. At the additional distance of 20 miles is another ridge (*e*) 500 feet high, followed by another hollow (*f*) from which the bottom rises gently. The Gulf Stream is parted into three streams by these two ridges. I once (1856) threw out the suggestion that these ridges might be formed by sedimentary deposit from the Gulf Stream itself. I now throw out another, and perhaps a more probable suggestion, in connection with the theory now under consideration. May not these wonderful ridges and hollows be parallel ranges of mountains *now in the course of formation* by the process described? Or else they may be *ranges formed* long ago *on the Atlantic border of the old eastern continental area*, as the Appalachian was formed on the interior basin margin of the same area. In this case, we may suppose they have become submerged in the partial subsidence of this continental area which subsequently took place.

(D.) *Metamorphism of mountain chains.*—From the evidence thus far brought forward, I think it almost certain that mountain chains are formed by the squeezing together and upswelling of lines of off-shore deposit. But the question naturally arises: *Why does the yielding to horizontal pressure take place along these lines in preference to any other?* I believe that the answer to this question is to be found in the recent views

* Prof. Bache, Proc. Am. Assoc., 1854, p. 140.

on the subject of *the aqueo-igneous fusion of deeply buried sediments.*

The accumulation of sediment, as first shown by Babbage, and afterward by Sir John Herschell, necessarily produces a rise of the geo-isotherms and an invasion of the sediments by the interior heat of the earth. From this cause alone, taking the increase of interior heat at 1° for every 58 feet, or about 90° per mile, and adding the mean surface temperature (60°), the lower portion of 10,000 feet of sediments must be at a temperature of about 230°, and of sediments 40,000 feet thick, like those of the Appalachian chain, must be nearly 800° F. Even the former moderate temperature, long continued in the presence of the included water of the sediments, would be sufficient to produce incipient change—at least *lithification*, if not metamorphism. In fact, lithification of sediments will probably take place under heavy pressure even at ordinary temperature, but is no doubt hastened by high temperature. The *latter* temperature of 800° is certainly sufficient to produce not only metamorphism, but aqueo-igneous pastiness, or even complete aqueo-igneous fusion. With a small quantity of alkali in the included water of such sediments, all these changes would take place at much lower temperature.

Suppose, then, sediments accumulating along the shores of a continent: The first effect is lithification, and therefore increasing density, and therefore contraction and subsidence *pari passu* with the deposit. Next, if the sedimentation continue, follows aqueo-igneous softening, or even melting, not only of the lower portion of the sediments themselves, but of the *underlying strata upon which they were deposited.* The subsidence probably continues during this process. Finally, this *softening determines a line of yielding to horizontal pressure*, and a consequent up-swelling of the line into a chain. Thus are accounted for, first, the *subsidence*, then the *subsequent upheaval*, and also the *metamorphism* of the lower strata so universal in great mountain chains. By this view, of course, the exposure of the metamorphic rocks on the surface is the result of subsequent erosion. Even the granite axis, I believe, in *most cases*, is but the *lowermost*, and therefore the most changed portion of the squeezed mass, exposed by subsequent erosion; although it is by no means impossible that in some cases the granite may be *squeezed out* as a pasty mass through a rupture at the top of the swelling mass of strata.

The theory, as will be observed, strongly inclines toward the metamorphic origin of granite, but does not require it. For there is nothing to hinder the aqueo-igneous fusion of an original granite crust by the accumulation of sediments upon it, and the consequent yielding of the crust along the line of accumulation.

(E.) *Fissures and slips.*—The enormous foldings of the strata which must occur in the formation of mountain chains by lateral thrust would, of necessity, produce fractures at right angles to the direction of thrust, or parallel to the folds, *i. e.*, to the range. The walls of such fissures would often slip *by readjustment by the force of gravity;* or else might be *pushed one over the other by the sheer force of the horizontal thrust.* The first case would give rise to those slips in which the foot wall has gone up and the hanging wall down, which are by far the most common slips in gently folded strata. The latter would give rise to those slips, often found in strongly folded strata, as in the Appalachian, in which the hanging wall has been pushed upward. The sudden rupture of the earth's crust, under the accumulating forces tending to bend it, sufficiently account for the phenomena of *earthquakes.*

(F.) *Fissure eruptions.*—The theory may, with much probability, be pushed so as to include volcanic phenomena also. There can be no doubt that the liquid and semi-liquid matters ejected by volcanoes vary in temperature and in kind of fusion in every degree from hot volcanic mud, through all stages of aqueo-igneous fusion, to pure or almost pure igneous fusion. Perhaps all the stages of aqueo-igneous fusion may be accounted for by the invasion of sediments and their included waters by the interior heat of the earth, as already explained. But the enormous temperatures often found in lavas cannot thus be accounted for. But it seems not unlikely, nay, even almost certain, that the invasion of sediments by interior heat would induce slow chemical action, which might increase the heat to almost any extent, so as even to produce true igneous fusion. If these views be correct, then beneath every great line of sediments, such as the off-shore débris of a continent, there exists a mass of partially or completely fused matter. When the line of sediment yields, and the strata are folded and fissured, the underlying fused mass is squeezed into the fissures to form dykes, or through the fissures and outpoured upon the surface as great *fissure eruptions*, which sometimes form the great mass of mountain chains.

(G.) *Volcanoes.*—There can be no doubt, I think, that the foundation of a true scientific geology was first laid by Lyell, in the study of "Causes now in operation." Nevertheless, the assimilation of agencies in previous geological times to those now in operation may be carried too far. As an example of this, I would mention the tendency among the most careful geologists to make our present volcanoes the type of all igneous ejections in all times. But I think no one who has examined the so-called volcanic rocks on this coast, both in the Sierras and in the Coast chain, but especially in the former,

can for a moment imagine that these immense floods of lava have issued from craters. The lava floods of the Sierra and Cascade ranges are, it seems to me, among the most extraordinary in the world. Commencing in middle California as immense but separate lava streams, in northern California it becomes an almost universal flood several hundred feet thick; in Oregon the flood becomes universal, and at least 2,000 feet thick, and this continues through Washington Territory and into British Columbia, how far I know not. An area 700 to 800 miles long and 80 to 100 miles wide seems to be almost universally covered with lava, and the thickest part where it is cut through by the Columbia river is not less than 2,000 to 3,000 feet thick. Over this immense area are scattered a dozen or more extinct volcanoes—mere pimples on its surface. It is simply incredible that all this lava has flowed from these volcanoes. There is no proportion between the cause and the effect. I am compelled to adopt the view of Richthofen* and of Whitney, that such great masses of lava, often constituting, as it does in this case, the *chief bulk of mountain chains*, have come, *not from crater eruptions, but from fissure eruptions*,—and that volcanoes are only secondary phenomena produced by the access of meteoric water to the still hot interior portions of these great fissure eruptions. Thus, as monticules are parasites on volcanoes, so are volcanoes parasites on massive eruptions, and massive eruptions themselves parasites on an interior fluid mass. This interior fluid mass, however, according to Richthofen and Whitney, is the supposed *universal incandescent liquid interior*, while I believe it is *the sub-mountain reservoir* locally formed as above explained.

By this theory, as by every other theory of mountain formation, it is necessary to suppose that there have been in the history of the earth *periods of comparative quiet*, during which the forces of change were gathering, and *periods of revolutionary change*—periods of gradually increasing horizontal pressure, and periods of yielding and consequent mountain formation. These latter would be also periods of great fissure-eruptions, and these, during the more quiet subsequent period, would be followed by volcanoes gradually decreasing in activity. The last of these great fissure-eruption periods on this coast was the Post-tertiary. The great lava flood which forms the Cascade range, where it is cut through by the Columbia river and its tributaries, *is every where underlaid by the northern boulder drift.*† Since that time we have been in what might be called a crater-eruption period, which was once extremely active but has gradually decreased, until now only geysers and solfataros remain.

* Richthofen, Natural System of Volcanic Rocks; Memoirs of Cal. Acad. Science, vol. i, part 2d.

† I hope soon to give the evidence of this in a separate communication.

I confess I do not see how either volcanoes or massive eruptions can be accounted for, except by the mode now explained. It is now, I think, generally conceded that lavas and other igneous ejections, at the time of their ejection, were in most cases only in a state of aqueo-igneous fusion, and therefore, cannot be regarded as evidences of the interior liquid. It must also be conceded that the focus of earthquakes and volcanoes are too superficial to have any immediate connection with an interior liquid, supposing such to exist. Volcanoes therefore must have their origin either in locally formed accumulations of liquid, as maintained in this paper, or else in local extensions of the general interior liquid, partially or entirely isolated within the solid crust.

In regard to fissure-eruptions, nothing but general contraction and a squeezing out of liquid matter can account for them. Whitney* thinks this squeezing out the result of subsidence of areas on either side of the mountain chain. I confess I do not understand the mechanics of this. Of course it could not be subsidence by weight, for this is inconsistent with the principles of hydrostatic pressure. It could only be by a concave bending of a stiff crust pressing on a fluid interior; but this over a large area is impossible, for the reasons already given in the early portion of this paper. Besides, pressure on a general interior liquid would be propagated equally to every portion of the interior surface of the solid crust, which would therefore yield not necessarily in a contiguous part, but at the weakest point wherever that may be. In fact, if we admit the interior fluidity of the earth, the mechanics of igneous agencies is surrounded with insuperable difficulties on every side. The more we try to arrive at clearness the more the difficulties seem to accumulate.

The theory which I have just presented accounts, it seems to me, for all the principal facts associated in mountain chains. This is the true test of its general truth. It explains satisfactorily the following facts. 1. The most usual position of mountain chains near continental coasts. 2. When there are several *ranges* belonging to one system, the ranges have usually been formed successively coast-ward. 3. Mountain chains are masses of immensely thick sediments. 4. The strata of which mountains are composed are strongly folded, and where the materials are suitable, affected with slaty cleavage; both the folds and the cleavage planes being usually parallel to the mountain chain. 5. The strata of mountain chains are usually affected with metamorphism, which is great in proportion to the height of the mountains and the complexity of the foldings. 6. Great

* Mountain building, etc., p. 90.

fissure-eruptions and volcanoes are usually associated with mountain chains. 7. Many other phenomena—such as fissures, slips, earthquakes, and the subsidence preceding the elevation of mountains, it equally accounts for.

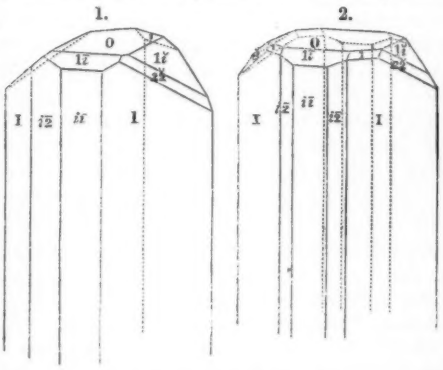
It will be remarked that the theory, though in its general features, not dependent upon, yet strongly inclines toward and is powerfully supported by, the views of Rose, Bischof, Hunt and others as to the metamorphic origin of granite and even of igneous rocks; the view that surface materials have passed by perpetually repeated cycles, through all the stages of rocks and soils; igneous rocks disintegrated to soils, carried away and deposited as sediments, consolidated into stratified rocks, metamorphosed into gneiss, granite or even into lavas, to be again after eruption reconverted into soils and re-commence the same eternal round; and thus we look in vain for the *original material*. I confess I lean strongly to this view.

I am fully aware that there are some phenomena of movement of the earth's crust which are not explained by the foregoing theory. I refer especially to those great and wide-spread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata. The last of these great oscillations took place during the Post-tertiary period. I cannot explain these oscillations. I am also painfully aware that the theory just presented, rests upon an insufficient knowledge of the structure of the earth. It is possible that the state of knowledge is not yet such as to warrant any attempt at a general theory. I feel quite sure that a perfect or even a satisfactory theory is not yet possible. I can only hope therefore that the theory here brought out may at least look in the right direction and will therefore serve as some guide in further investigation; that amid the modifications which theoretic geology must undergo in the advance of knowledge, some remnants of its outline will still remain visible. In any case, even if entirely wrong, it is at least a little more definite than anything we have. It is at least something tangible which may be attacked and overthrown by facts and by physical reasoning. We have had enough of vague theorizing in geology; of vague shadows through which the trenchant sword of science passes with no effect. It is time that the more perfect methods of physics were applied to geology.

Oakland, Cal., May 15, 1872.

ART. LVII.—*On a crystal of Andalusite, from Delaware Co., Pa.;* by EDWARD S. DANA.

THE annexed figures represent a remarkable crystal of andalusite, from Upper Providence, Delaware Co., Penn., received by Professor Dana from Dr. George Smith, and now in the Yale College Cabinet. Figure 1 shows the crystal (natural size) with the planes as actually occurring. It will be noticed that while all the known planes, with one exception ($i\bar{2}$), are present, there is an irregularity in their occurrence almost amounting to a kind of hemihedrism; 1-$\bar{1}$ and $i\bar{2}$ appear but once, instead of twice, and $i\bar{2}$, $2\bar{2}$, and 1 once, instead of four times. Figure 2 shows the crystal in its theoretical form, with all the planes as they would regularly occur. $I \wedge I$ gave 88° 15′ (that is, 91° 45′), and $O$ on the macrodiagonal section the angle 93° to 94°; this obliquity, however, is not in the right direction to explain the partial hemihedrism.




By the kindness of Mr. Vaux and Mr. Joseph Willcox of Philadelphia, I was enabled to examine other fine specimens from the same locality. The number of planes occurring on these crystals (one of which weighed more than 7 lbs.), was small, and there was nothing in their manner of occurrence to suggest that the peculiarity of the crystal figured was anything more than an accidental irregularity. In all of these specimens there was a great diversity in the prismatic angle, and obliquity in the angle of $O$ upon the diagonal sections was very common.

A word should also be added in regard to the cleavage in the specimens from Delaware Co. In most cases it was irregular, many of the crystals having a fibrous, tremolitic structure, and in others it was radiated. The regular cleavage parallel to the prismatic faces, however, did occur, and a chemical analysis of several of the specimens is needed to determine whether in the former case any change in constitution had taken place.

ART. LVIII.—*Spectrum of Lightning;* by EDWARD S. HOLDEN, Lieut. of Engineers, U. S. Military Academy, West Point.

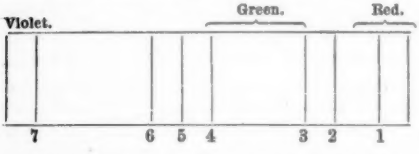
I DESIRE to communicate to you a few observations on the spectrum of lightning, which I could wish to be more complete. The instrument was a pocket spectroscope of Hawkins & Wales. The first set was made in Philadelphia shortly after sunset, on the evenings of August 13, 14, and 15, 1872. There was a continuous play of sheet lightning and frequent vivid flashes.

In the sheet lightning and in the fainter flashes the green and blue portions of the spectrum were visible, the violet and red cut off; in the brighter flashes a complete and continuous spectrum appeared and superposed on it bright lines. The red end of this spectrum (of vivid flashes) seemed to be shorter than that of the spectrum of a common gas-jet turned down low, with which it was constantly and almost instantaneously compared, without moving from the place of observation.

From the sheet lightning I repeatedly obtained series of bright bands in the green, but the width and intensity of these bright bands continually changed. Of the bright and sharp lines I saw but three:—1, line in green; 2, line in blue; 3, line in violet (or extreme blue?).

These were seen frequently, and sometimes those of one flash would be immediately succeeded by those of the following flash, thus giving me a means of assuring myself that the *same* lines appeared as well in *position* as in color.

The second set I regard as trustworthy. The observations were taken at West Point, N. Y., August 22d, 1872, at 6.00 P.M., looking toward the east through a violent rain. The spectroscope was first directed toward the sky and the spectrum with dark lines, which was constantly seen, was mapped as below, and the following notes taken:




*Notes*—The red ends slightly beyond 1.
2 = D line,
3 bounds yellow line.
5 near boundary of green.
6 seems to be sometimes green and sometimes blue.
7 violet = H.

The spectroscope was then turned to the lightning, and with the above dark lines as reference lines the following bright lines were mapped: (*a*) bright line *less* refrangible than red end (border) of spectrum, i. e., *extra red;* (*b*) bright line slightly *more* refrangible than 4 (see fig.); (*c*) bright line near 5 or 6 (fig.) "green *or* blue"; (*d*) bright line in blue between 6 and 7, once seen, bright purple.

The green portion seemed to have variable limits and to be disproportionately bright, but no green bands were seen. In my note book I have marked (*a*) (*b*) and (*c*) "sure of."

U. S. Mil. Acad., West Point, N. Y., Oct. 9, 1872.

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*Letter from* Dr. B. A. GOULD, *Director of the Observatory at Cordoba, to the Editors*, dated Cordoba, Sept. 4, 1872.

Scarcely a mail has gone out homeward for many months without my having experienced a strong desire to tell you of our progress, yet I have allowed sixteen months to pass, rather than repeat the old story of obstacles and delays; for, although we have all worked to the utmost of our power, this interval has served to show how erroneous were my estimates of what could be accomplished within a given interval, in a new country and at a distance from those facilities to which we are so thoroughly accustomed at home that it is difficult to feel how indispensable they are, or to make allowance for their entire absence. And while anxious to fulfill my promise of writing to you, I was unwilling to send tidings unaccompanied by accounts of something done toward the fulfillment of my original plan.

It lacks but a few days of two years since my arrival in Cordoba, when I anticipated that the work upon the zones would commence within six months and be completed within two years thereafter. Yet the sound of the hammer is still heard within the building; and the two years will have elapsed before the carpenters can take their leave. But if the institution thus established, and devoted to astronomical research in this clear and transparent sky, can now commence an era of full activity, I shall not feel that these years of toil in the joint capacity of architect, surveyor, master-builder, engineer and mechanician, as well as astronomer, have been in vain.

The various unexpected embarrassments, which have till now delayed the commencement of the zone observations, have not interfered with the Uranometry, which has advanced as uninterruptedly as moon and clouds would permit. During the first fourteen months of our sojourn, the survey of the entire sky south of the 10th degree of north declination was completed, the stars identified from the catalogues and their positions reduced to the mean equinox of 1872·0; or for those few which could not be identified, their positions fixed by estimation with sufficient sharpness to permit of recognition and accurate determination when the large instruments should be in working order. The scale of magnitudes above the 6th was carefully deduced from that of Argelander within the belt between 5° and 15° of N. declination and expanded to tenths of a magnitude. For stars below Argelander's limit, we had no trustworthy basis; and although a very large proportion of our stars were noted as 7 by Lacaille, and many even fainter than 7 by Lalande, Taylor and Brisbane, I was

indisposed to believe that stars really below 6½ or 6·6 could be visible to the naked eye. Experiments made with considerable minuteness, to deduce the true magnitudes by determining the smallest aperture through which the stars could be distinctly seen, failed to give a satisfactory result, and thus I fixed upon 6·5 as the limit of average vision on an average night; and upon this basis the magnitudes below 6 were estimated, stars seen by the unaided eye being considered as below 6·7. The work had far advanced before the arrival of any means of accurately testing the exactness of this assumption; and my suprise was great when after a comparison of the faint stars within the type-belts with Argelander's "Durchmusterung" and Bessel's zones, there remained no room for doubt that the magnitude which we had been calling 6·5 was in reality not more than 7·0; and that on the clearest nights stars not brighter than 7·2 could be distinctly seen, while a considerable number which had been seen and recorded are not above the 7·5 magnitude. This is beyond all question, and indicates at once the transparency of our sky, and the sharp sight of our observers.

At first I had fixed upon the magnitude 6·5 as the most appropriate inferior limit for stars to be included in the Uranometry, but this discovery led to a total change of plan, and I now think that the limit ought not to be brighter than 7·0. It was not difficult to translate the magnitudes recorded into the corresponding true ones, since most of the stars had been observed two or three times, and by two or three observers; still as the progress of the building did not yet permit the erection of the meridian circle, I decided to submit the sky to a new revision. In the first scrutiny the whole region included in our work had been divided into 17 maps; 5 of which were assigned to Mr. Thome and 4 each to Messrs. Rock, Davis and Hathaway. The boundaries were so arranged that each of these maps lapped very largely upon the adjacent ones (which were of course assigned to other observers), so that there were few stars which did not fall within the limits of at least two observers, while there were several regions which belonged to the common domain of all four. Thus the scrutiny seemed good in so far as regards the detection of all stars within the limits of vision; while any systematic tendency to diversity in the estimates of the several observers could be at once recognized and remedied. The diversity of results in consequence of errors of observation or differences of judgment was small, and in most cases indicated that the true value was an intermediate one.

Nevertheless as I have said, a revision of the whole work has been undertaken, which is a far less severe undertaking than the original one, inasmuch as the stars have been already identified and their places reduced to the adopted equinox. For this revision a large number of additional faint stars have been added to the catalogue in the type-belt—those only being adopted as standards of magnitude to which all four observers assigned the same value. The scale thus established has been similarly transferred,

by the accordant estimates of all, to two regions on opposite sides of the pole, and at about 60° declination, so that an abundant series of trustworthy standards of magnitude may be found within a convenient distance of any part of the sky. For the revision with this new scale, I divided the hundred degrees of declination which compose the field of our Uranometry into 35 charts, each mapped upon a scale nearly double that of the first series, which was easily attained by diminishing the amount of their overlapping. These have been so distributed as to assign to each observer regions different from those which he had previously observed, and each one is now engaged in repeating his former work, with the new standards of magnitude, and in observing the new charts of the revision series. Thus I think we may believe that no star brighter than the 7th magnitude will escape notice, that the misidentifications will be few, and that the final results for the magnitudes deduced from so large a number of observations, free from systematic discordance, will be entitled to a high degree of confidence.

This revision, as well as the repetition of the original work, are both of them more than half completed, and I see no reason to feel otherwise than very well satisfied with the results. My great nearsightedness has prevented me from taking any personal share in the observations, a circumstance which at first caused me deep regret; but this regret has disappeared since experience has taught me that the amount of labor entailed by the general arrangement of the work, and by the combination and scrutiny of the results, is quite incompatible with any considerable amount of direct observation. And I doubt whether this could have been more zealously or faithfully accomplished than by the gentlemen engaged in the work.

Of course this process has brought to light a considerable number of variable stars, of which I had hoped to be able to prepare a list before now; and has moreover served to fix reasonable suspicion upon a much larger number. Before long I trust to find opportunity to collect and arrange our data. You may well suppose that in the pressure of our labors there has been small opportunity to follow up the variations of these stars, so as to determine their periods. That duty must be left for a season.

Among those which have come to light I may mention one especially, since it belongs to the northern hemisphere. It is the star in *Monoceros*, No. 507 of the Hour VI. in Weisse's Bessel, the place for 1872·0 being

$$6^h\ 18^m\ 19^s + 7^\circ\ 9'\cdot 2.$$

Bessel called its magnitude 7, and Mr. Davis noted it as 6·1 in the beginning of 1871; but his subsequent observations have shown it to fluctuate between the limits 6·2° and 7·3° in a period of about 31 days.

But enough of the Uranometry, which must soon be brought to a conclusion.

With the meridian circle I have already accomplished a very fair amount of work in determining the positions of stars uniden-

tified for the Uranometry, and of others regarding which there is discordance in the existing catalogues. The observations for latitude are completed and will give a value which can probably not be essentially improved without an investigation of the division errors of the circle. I have not yet completed their discussion, but the resultant latitude will not differ much from

$$-31^\circ\ 25'\ 15''.4.$$

In connection with Señor Moneta, Chief of the Corps of National Engineers, I have already carried out two series of longitude-determinations; the one with the city of Rosario, the other with Buenos Aires. With each of these places time-signals have been exchanged on several nights, and with results that indicate that Cordoba is in fact more than a minute of time to the westward of the position given it on the best maps. The telegraph across the Andes to Chile was completed about a month ago, and Professor Vergara, of the National Observatory at Santiago, is now constructing a branch line to his observatory for the purpose of effecting a series of longitude measurements with Cordoba. And since the longitude of Santiago de Chile from European meridians is doubtless better determined than that of any other point in South America, the proposed undertaking should not only give us a very trustworthy result for this observatory, but likewise improve the adopted values for Buenos Aires, Rosario and Montevideo. The value which I am for the present adopting is

Cordoba $0^h\ 51^m\ 33^s$ E. from Washington,
or $4\ 16\ 39$ W. from Greenwich.

All obstacles to the commencement of the zone-work have, I think, at last been overcome, and I am already practising the preliminary drill. The last hindrance was a peculiarly vexatious one. There is no convenient place in our meridian room for the proper support of an astronomical clock, since my plan contemplated a clock mounted upon a pier in another room, and provided with a telegraphic dial on the same plan as those which I employed at the Dudley Observatory in 1857. Upon unpacking the dial and placing it in position, I found that the one sent me could only be actuated by a commutator reversing the current every second, while the clock itself was only furnished with a break-circuit arrangement. Elsewhere the embarrassment might not have been serious, but here it was otherwise. I might have sent home for another dial, or a fitting commutator, but this would have entailed a delay of at least four or five months; so, ignoring the want of local opportunities, we have been struggling since May in an attempt to construct one of the "home-made" sort, which should be sufficiently nice in its mechanical execution never to miss a second, and yet interfere as little as possible with the clock rate. This is at last accomplished, thanks to the persistent efforts of Dr. Sellack, and the dial is now performing its functions in a satisfactory manner.

With the equatorial I was able to follow the comet discovered by Tempel Nov. 3, 1871, on every clear night but one from Jan. 17 to Feb. 21, in spite of its extreme faintness. This comet was observed in Europe only for about ten days, and I think the Cordoba observations will prove to be the only ones made in this hemisphere. The comet traversed the southern hemisphere, passing within about 5° of the pole, so that it has not yet been possible to determine all the comparison stars with the circle; but as soon as possible I hope to complete the series of determinations. It is on such occasions that one feels most keenly the intensity of the existing need of a southern catalogue; for of all the comparison-stars employed, I succeeded in identifying only two as having been previously observed.

Among my most cherished plans in connection with this expedition has been that of obtaining photographic impressions of prominent star-clusters in this hemisphere, for measurement and computation of the same kind as that bestowed, before leaving home, upon Mr. Rutherfurd's photographs of the *Pleiades* and *Præsepe*. With this view I made an earnest but unsuccessful effort in Boston to obtain the needful means by subscription. But in December last some of my near connections supplied the necessary funds for the expenses and equipment of a trained photographer, and through the exertions and friendly care of Mr. Rutherfurd, the services of Dr. C. S. Sellack were secured for a limited period, and an adequate supply of chemicals and apparatus provided. Dr. Sellack left the United States in December, but having been separated from his boxes in Rio Janeiro did not reach Cordoba until the middle of March. As soon as possible a little photographic laboratory was constructed, and after a little less than a month the photographic object-glass was unpacked from the box in which it had remained since leaving New York. To our dismay, on unpacking the glass, the flint lens was found broken in two with a cruel fracture of irregular form, which divided it into two not very unequal segments. Since then we have been engaged in constant efforts to render it serviceable; but were it not for the extreme ingenuity and unwearied persistency which Dr. Sellack has brought to the work these endeavors must have proved fruitless. Month after month our faith and hope have dwindled, as one device after another failed to attain the end desired, or developed some new difficulty in the way; but I am so happy as to be able to say at last that the difficulties seem to be essentially surmounted, so that I do not despair of accomplishing something after all. A photograph taken three days ago gives most encouraging prospects, for we have on a single plate images of thirty-six stars in the cluster in Scorpio, and although the images of the brighter stars are slightly elongated, they are certainly good enough to afford accurate results. And as the chemicals were accompanied by the priceless gift from Mr. Rutherfurd of the beautiful micrometer which has in his own hands done such exquisite work, I cherish some hope of being able to send

you the palpable demonstration that by patience and ingenuity something may be done even with a fractured lens.

The great scientific importance of a study of the singular meteorological relations of this country has made me unwilling to neglect any opportunity of furthering such investigations; although greatly indisposed to sacrifice any time which might be devoted to astronomical researches. With this feeling I have lost no opportunity of urging on the Government the high importance of an organized system of meteorological observations, and a bill is now pending in the Argentine Congress, with every prospect of a speedy passage, to establish a Meteorological Bureau, and to provide competent observers in various parts of the country with the necessary instruments. I have provisionally undertaken the organization and management of this Bureau, but with the hope of being able before long to resign it into some competent hands.

I have also undertaken the Commissionership of Weights and Measures, hoping thus to contribute something toward the furtherance of the great international movement toward the unification of weights, measures and currency. And I am glad to announce that as a preliminary step toward the practical introduction of the metric system, it has been ordered by the Government that from and after Jan. 1st, 1873, all the measurements and records of the custom houses of the nation are to be made in metric units. At present every one of the 14 provinces has its own measures of length and capacity, each differing from the others, and all differing from those of Spain, whence they were derived. It will not be a difficult matter, I am convinced, to bring the metric units into practical use throughout the country.

Of other scientific news I have but little to tell. A very beautiful meteor passed over the city of Tucuman at about $5\frac{1}{2}$ A. M., on the 21st August, dazzling those who were in the streets, and alarming them not a little. It exploded with a loud report. The motion is said to have been toward the east, but no more definite information could be obtained.

We are in the midst of the tempestuous but rainless season of the year. Two or three times a week, hurricanes pass over the city, rendering the air opaque with dust and doing much injury to trees and houses. This is one of our great troubles, in consequence of the harm done to the instruments by the penetrating clouds of fine hard clay-dust. There has been no rain for many months, and the bed of the Rio Primero is dry, below the upper part of the city; a not very uncommon phenomenon.

# SCIENTIFIC INTELLIGENCE.

## I. Chemistry and Physics.

1. *On Manometric Flames;* by Dr. R. König, of Paris.—[The following is a translation of the latter part of Dr. König's admirable paper "On manometric flames," recently published in *Poggendorff's Annalen*, Bd. 146, S. 165. The figures annexed are from electrotypes which accompanied the acoustic apparatus recently sent by Dr. König to the Stevens Institute of Technology. —A. M. Mayer.]

*Interference Phenomena.*—In describing the results obtained by the combination of the notes of two organ pipes, I did not make

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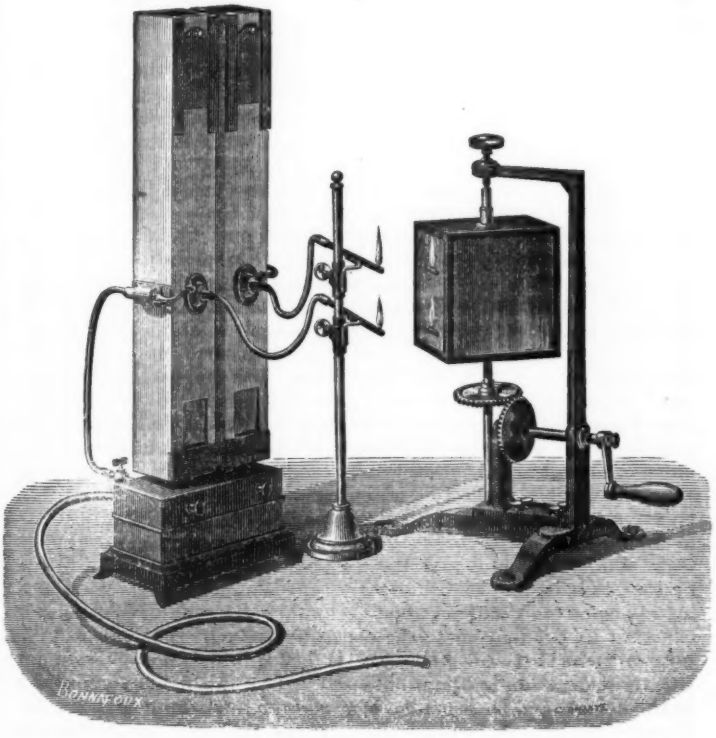


mention of unison. The combination of two notes in unison is of special interest on account of the communication of the vibrations and the attending phenomena of interference. I therefore prefer to describe them in connection with other similar experiments.

If we take two organ pipes in unison, attach to them two flames, and sound one of them, the flame of the other will show that the included column of air has had the vibrations communicated to it, and that this communication continues even when the organ pipes are no longer exactly in unison, but give beats when sounded together. We must remark, however, that in this case, the second pipe has no vibrations of its own, but only such as are exactly in unison with the first, so that the beats can neither be heard nor observed in the flame. If we now sound the second pipe also, and thus induce vibrations of its own, these will combine with the resonance vibrations, and the flame violently indicates beats which can also be distinctly heard.

I draw particular attention to this isolated appearance of resonance vibrations in the column of air, because this does not happen in the case of two violin strings stretched over the same sounding board, where the string vibrations are always combined with the resonance vibrations in the influenced string, even if it is not sounded. The beats produced by two such strings acting on each other are of such nature that one reaches the maximum amplitude while the other is at the minimum. The flames of two organ pipes show the same phenomenon, one rising while the other falls. In the latter both must be sounded, while it is necessary to sound only one of the strings.

In pipes of perfect unison whose vibrations make the same mutual compensation that the beats did before, that is to say, where condensations take place in the node of one while rarefactions take place in the node of the other, we can plainly observe the whole process by means of two flames, one placed vertically below the other. The vibrations of the flames are undiminished, but their images in the revolving mirror alternate instead of being one under the other. When both notes act upon the same flame, the flame will, in the case of beats, be more violently agitated than the two flames were; for in the latter case they were caused both by direct and by induced vibrations, which in the same column of air were of very unequal intensity; here, however, two notes of almost equal intensity are produced directly in two equal columns of air. If the two notes are made gradually to approach unison we shall observe that we cannot retard at will the beats as in the case of tuning forks; but when we come to a certain point they suddenly cease, and the two columns of air vibrate like a system, that is, like two differently tuned bodies which are so intimately connected, and act upon each other so strongly, that neither can emit its peculiar note; the consequence being that but a single note, a mean between the two, is heard. This note is stronger than that of a single organ pipe and causes the flame to contract in the center, and to rise above a non-luminous blue broad base. As we approach pure unison the height of this dark base increases, the luminous contraction disappears and when unison is reached the flame appears at rest. At the same time the strong fundamental note of the organ pipes has almost entirely disappeared,

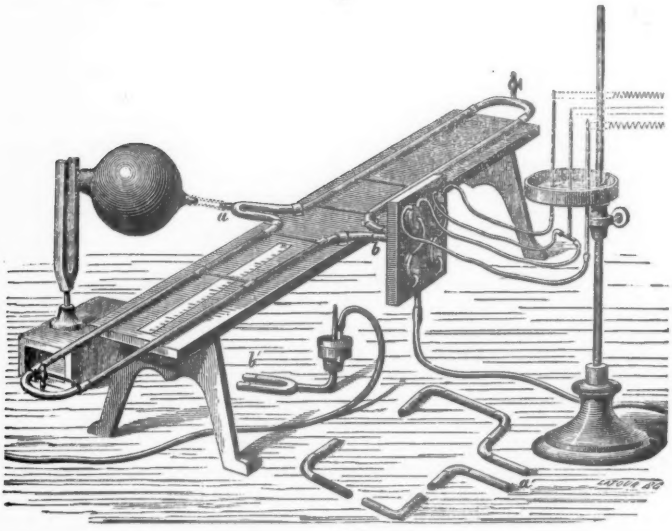
and we clearly perceive the first overtone, since, as we well know, the even overtones are strengthened and the uneven ones destroyed whenever the difference of half a phase of vibration occurs in the two notes in unison. This octave [the first overtone] is also seen in the flame which produces a series of low broad images in the mirror each of which is cleft. It is well in this experiment to use greater pressure of the air in order to increase the intensity of the octave in the pipe.

Since this appearance of the octave in the interference of two fundamental notes may be shown very beautifully also by means of the double syren of Helmholtz, I also made this phenomenon visible by means of the flames. For this purpose I provided each of the two resonant boxes, which are over the revolving discs, with a tube which permitted me to bring their interior in direct communication with the tube leading to the manometric capsule; which I did by means of gum tubes in such a manner that the upper wind box of the syren preserved a limited mobility so as to produce and interrupt interference through its different positions. Whenever we approach the point of interference of the upper wind box we see the large vibrations of the fundamental tone gradually disappear, and the short cloven flame of the octave take its place.

A special apparatus, which I constructed for observing phenomena of interference of different kinds, is based upon a method first used by Herschell, and after him by other physicists. The principle consists in producing interference by allowing waves coming from the same source of sound to traverse two different directions, differing in length by half a wave-length, and then uniting them again. A tube is used branching off in two directions, the branches being so constructed as to allow lengthening and shortening. (Fig. 2). In order to obtain a very complete interference, we must introduce as simple a note as possible into the tube by connecting it with a resonator before which the corresponding tuning-fork is sounded. If we now lengthen one of the arms until the difference between them amounts to half a wave-length of the note of the tuning-fork, the waves coming through the two conduits destroy each other at the other end of the tube, and if we allow this to terminate in a small cavity, above which is a manometric capsule, we shall see, on lengthening one of the arms, how the deeply cloven flames in the revolving mirror are gradually changed into bands of light when the difference of half a wave-length is reached. Interference may be shown much more beautifully by means of another arrangement. Instead of allowing the united arms to act upon the capsule, I apply to the ends of them a small apparatus connecting them with separate capsules. These two capsules, whose mutual action is canceled by the two auxiliary capsules, are connected with two gas pipes instead of one. On a stand are three burners of different heights, the middle one being provided with two gum tubes. I now connect one gas pipe of one capsule with the highest burner, one pipe of the

other capsule with the lowest, and by means of the other two pipes I connected both capsules with the middle burner. If I now

2.



sound the tuning forks, both branches being of equal length, the three flames in the revolving mirror appear as three series of flames cloven to an equal depth one above the other. On lengthening one branch half a wave-length of the note, the middle one alone becomes a simple band of light, while the other two continue to vibrate with unchanged intensity. Here we observe at the same time the effect produced by waves of sound coming through each arm separately and when they are reunited.

If in these experiments we use an open organ pipe, instead of a tuning fork with resonator, the vibrations of the octave appear again during the interference of the waves of the fundamental note; provided the organ pipe is not of too great diameter. In the same manner as the fundamental note we can also remove any overtone of a note by means of interference. This can be shown very nicely by means of a closed pipe. I introduce its sound into the apparatus by connecting it to the latter by means of gum tube attached to its terminating capsule, after removing the gas burner. If I then pull out the tube so far that interference is produced for the note 3, the middle flame in the mirror will show the simple flames of a fundamental note, while the other two will show the same image as in the combination of the notes 1 and 3. In the same manner we can separate whole series of overtones from the vowel sounds, and this forms a very fruitful method of studying these sounds. In these experiments

the arrangement of the three flames is particularly useful, because the constant images of the upper and lower flames render the slightest variations of the middle flame very perceptible. If U [German = oo] is sung to the note $\bar{c}$ in the apparatus, the fundamental tone is but weakly accompanied by the octave; if we then arrange the apparatus so that the waves of $\bar{c}$ interfere, every trace of this octave disappears; while on interference of the fundamental note two narrow flames of almost equal height take the place of each broad one, which represents the octave which exists now almost alone. If we sing O to the same note, where the octave is stronger than with U, we can make the same experiments, but here the tone 3 appears on interference of the octave, the broad flame of the fundamental note changing into three points successively diminishing in altitude. If the waves of the octave interfere we get a group of five peaks of flame which indicate the tones 1, 3, 5. If we suppress the fundamental tone and with it the tones 3, 5, etc., we get a simple series of flames caused alone by the octave. These phenomena are not, however, always so simple, especially in more composite groups of flames pertaining to the lower notes. I will therefore remark, that on lengthening one tube of the apparatus we often suddenly see very considerable changes in the images of the flames, while it is between the points of interference of two successive overtones of the note. It is then the point of interference of the lower octave or 12th of a higher overtone of the note which is separated in this manner.

Instead of the branching tube, in which the sound was introduced in the preceding experiment, we can use two separate tubes exactly equal in length and shape, each of which consisting of three pieces united as in a telescope, so that the two open ends may be turned in any direction without change of length or curvature. This apparatus allows a sound, proceeding from two parts of the vibrating body to be introduced into the apparatus; as, for instance, from parts of a plate having different signs, or from the two sides of the same part. In these two cases interference takes place on passage of the sound through the two conduits of equal length, and the sound appears only when interference is destroyed by pulling out one arm. In order to make the apparatus do for determining the wave-length of a tone in different gases, and for the experiments of Zoch, I have provided the tubes with two stop-cocks by which they may be filled and emptied. When we experiment with a gas other than the atmospheric air the resonator cannot of course remain connected with the interior of the tube, and we must therefore introduce a small hollow vessel separated in the middle by a membrane. One half is connected with the tube the other with the resonator. Besides this we must cover the ends of the tubes with india rubber rings to prevent the gas from escaping at these points.

Of course this apparatus will do for direct observation of the different phenomena of interference with the ear, and for repeating the experiments of Mach, Quincke and others. For this pur

pose we must put in the place of the flame apparatus one of the forked tubes and connect this with the ear by means of an india rubber tube.

2. *On the light emitted by the phosphorescent compounds of uranium.*—BECQUEREL has examined the phosphorescence spectra of some of the compounds of uranium, and has arrived at the following results:

(1.) The compounds of protoxide of uranium hitherto studied (chloride and sulphate) did not exhibit any appreciable phosphorescence. But although some compounds of the sesquioxide are equally inactive, this is not the case with the greater number, which when properly treated give rise to a more or less vivid emission of light.

(2.) The greater number of these phosphorescent substances give a series of groups of luminous and dark bands which appear in a part of the spectrum extending from about C to beyond but near F. These groups are 5, 6 or 7 in number, and the bright and dark bands formed by them are not in the same places for the different compounds, but preserve the same positions in the case of the same substance.

(3.) If the succession of luminous groups in the spectrum characterizes in general the compounds of uranium, the acid in the compound determines the disposition of the bright and dark bands of each group, which disposition may differ greatly for the different compounds.

(4.) In the double salts of the same class, in the sulphates and double sulphates for instance, the composition of each group remains the same, but the index of refraction of the corresponding bright and dark bands is different. Sometimes these groups are transferred a short distance from the more refrangible or from the less refrangible side, according to the simple or compound uranic salt. Thus in the case of the double chloride of uranium and ammonium, the lines or bands are a little more refracted than the corresponding lines or bands presented by the double chloride of uranium and potassium, while the contrary is the case when we compare the double sulphates of the same bases. With the double oxalate of ammonia and uranium, the groups similar to those which the simple oxalate gives are less refracted than in the case of this last, and the difference is greater than that observed with the sulphates.

(5.) If we consider the characteristic lines or bands of each group in the same compound (which may be the center of a bright band or a dark line), we find that from the first group to the seventh, the distance measured by the aid of the micrometer of the spectroscope increases with the refrangibility; on the other hand, the differences between the wave-lengths of the corresponding luminous rays diminishes; the ratio of these differences to the mean wave-lengths also diminishes, but the ratio of these same differences to the squares of the mean wave-lengths changes little, changes for the same compound between the extreme degrees of refrangibility,

and may be regarded as sensibly constant. Moreover, with different compounds this ratio only varies between limits but little removed from each other. Thus we have for the mean value of this ratio:

| Substance. | Ratio $\frac{d}{\lambda^2}$ |
|---|---|
| Chloride of uranium, | 0·000081 |
| Chlor. of uranium and potassium, | 83 |
| Fluor. of uranium and potassium, | 81 |
| Sulphate of uranium, | 85 |
| Sulphate of uranium and potassium, | 84 |
| Oxalate of uranium, | 86 |
| Phosphate of uranium and lime, | 82 |
| Nitrate of uranium, | 88 |
| Arsenate of uranium, | 83 |

(6.) There does not appear to be any simple relation between the wave-lengths corresponding to the homologous lines or bands of the same luminous group in different compounds and the chemical properties of these substances.

(7.) When we illuminate the solid compounds of uranium with transmitted violet or ultra-violet light, we observe in the most refrangible part of the spectrum groups of absorption bands which differ for each compound, and which appear to correspond in this part of the spectrum to less refrangible groups of bright phosphorescent bands, and to continue their succession.—*Comptes Rendus*, lxxv, 296. W. G.

3. *On the spectrum of the Aurora Borealis.*—Vogel has made an attempt to identify the spectrum of the aurora with that of air, and has arrived at results which, if not absolutely conclusive, render the identification at least probable. The author employed a direct vision spectroscope with 5 prisms, collimator and observing telescope, which last by means of a micrometer screw could be moved so as to bring different parts of the spectrum into the center of the field of view. The distances between the spectral lines could be read off in fractions of a revolution of this screw. By repeated measurements of about 100 lines, aided by Angström's atlas, the micrometer readings could be converted directly into wave-lengths. With this instrument Vogel determined the wave-lengths of 7 lines with the following results:

| Wave-length. | Probable error. | Remarks. |
|---|---|---|
| 0·0006297 | 0·0000014 | Very bright band. |
| 0·0005569 | 0·0000002 | Brightest line. |
| 0·0005390 | | Very faint line. |
| 0·0005233 | 0·0000004 | Pretty bright. |
| 0·0005189 | 0·0000009 | Bright where the red line appears; otherwise faint. |
| 0·0005004 | 0·0000003 | Very bright. |
| 0·0004694<br>0·0004663<br>0·0004629 | 0·0000003 | Broad band somewhat less bright in the center; very faint where the red line appears. |

For the purposes of comparison the author determined the wave-lengths of the positive lines in oxygen, hydrogen, nitrogen and air, employing for this purpose Geissler tubes, the discharge being that of a weak inductorium. The spectra of both the narrow and the wide portions of the tubes were observed both as regards wave-length and intensity of light, and finally the spectrum of rarefied air saturated with aqueous vapor was also noted. The first auroral line ($\lambda = 629{\cdot}7$) appears to correspond with one of a system of lines in the spectrum of nitrogen, the wave-lengths of which range from 662·0 to 621·3. The brightest line in the spectrum of the aurora ($\lambda = 556{\cdot}7$) is found in the spectrum of nitrogen as a faint line. The line ($\lambda = 523{\cdot}7$) exists both in the spectrum of nitrogen and in that of air. The fifth line of the aurora corresponds with the third line in the spectrum of oxygen ($\lambda = 518{\cdot}9$). The sixth line of the aurora corresponds very accurately with the nitrogen line seen in the spectra of certain nebulæ ($\lambda = 497{\cdot}5$). Finally the broad band of light between 469·4 and 462·9 corresponds to several lines in the spectrum of nitrogen as well as in that of air. As the general conclusion to be deduced from his work, the author believes that the spectrum of the aurora may with great probability be regarded as a modification of the air spectrum, the variability of the spectra of gases under different circumstances of temperature and pressure being well established.—*Pogg. Ann.*, cxlvi, 569. W. G.

4. *On the heat of expansion of solid bodies.*—In the cases of a number of different solids and also of water, BUFF has compared the quantity of heat absorbed in producing expansion with the whole quantity absorbed, or, in other words, with the total specific heat. The author sets out with the extremely probable assumption that the coefficients of expansion and compression are equal, whether the expansion or compression be linear or cubic. A simple calculation, then, shows that in the case of iron, as an example, the quantity of heat necessary to raise the temperature of one cubic centimeter of iron, 1°·374 C., corresponds to a work of 100 kg. × 0·0000481 centimeters, or 4·81 centimeter-grams. This quantity of heat is found in units by multiplying the weight of 1 c. c. of iron, 7·757 gr., by the specific heat of iron, 0·1098, and by the increment of temperature, 1°·374 C., which gives 1·17 units. The total work which this quantity of heat is capable of producing is 42000 gr.-cm. × 1·17 = 49140 centimeter-grams, while the actual work of expansion is only 4·81 centimeter-grams. Hence the heat of expansion is to the total heat as 0·98 is to 10,000, or about 0·01 %. The following table gives the author's results, with the different substances to which the calculation was extended:

| | $\alpha$ | $\beta$ | $\delta$ | $s$ | $\lambda$ |
|---|---|---|---|---|---|
| Iron, | 0·0000481 | 0·0000350 | 7·757 | 0·1098 | 0·980 |
| Copper, | 0·0000951 | 0·0000515 | 8·936 | 0·0949 | 1·446 |
| Silver, | 0·0001401 | 0·0000573 | 10·301 | 0·0577 | 2·378 |
| Gold, | 0·0001791 | 0·0000466 | 18·035 | 0·0324 | 1·899 |
| Platinum, | 0·0000628 | 0·0000265 | 21·166 | 0·0324 | 0·920 |
| Lead, | 0·0005634 | 0·0000854 | 11·165 | 0·0314 | 5·800 |
| Glass, | 0·0001451 | 0·0000262 | 2·446 | 0·1770 | 1·441 |
| Water at 16° C., | 0·0045854 | 0·0001600 | 0·999 | 1·000 | 3·810 |

In this table the column $\alpha$ gives the cubic coefficient of extension referred to the millimeter as the unit of length; column $\beta$ gives the cubic coefficient of expansion for 1° C.; column $\delta$ gives the density of the body; $s$ its specific heat, and $\lambda$ the quantity of heat which becomes latent in ten-thousandths of the whole quantity. It will be seen that it forms a very small fraction of the whole. It is thus easy to understand why it has not yet been possible to raise the temperature of a solid by compression. We also see that the latent heat of expansion exerts a very small influence on the specific heat of the atoms of solid bodies.—*Pogg. Ann.*, cxlv, p. 626. W. G.

## II. Geology and Natural History.

1. *Wyoming Coal Formations.*—Prof. E. D. Cope describes, in an interesting paper read recently before the Am. Phil. Soc. Philad., a large *Dinosaurian*, discovered during the past summer by Mr. Meek and Dr. Bannister, at Black Butte Station, on the Union Pacific Railroad, in Wyoming Territory. In this, as well as in a later paper published in the *American Naturalist* on the age of the Wyoming coals, Prof. Cope remarks, that the determination of the affinities of this Saurian proves that these coals, which hold a lower position, belong to the Cretaceous age, and not to the Tertiary, and he writes as if all others had been in error on the age of the deposits. Prof. Cope was doubtless not aware that Mr. Meek had, in 1871, referred Dr. Hayden's collections from this formation on Bitter Creek, at Point of Rocks, to the Cretaceous;* and that this same careful paleontologist had also referred the coal-bearing rocks of the same great series at Coalville, Utah, and at Bear River City (Sulphur Creek), Wyoming, to the Cretaceous in 1870, as did also Mr. King and Mr. Emmons.† Indeed, as long back as 1860, Mr. Meek, in connection with Mr. Engelmann, referred Capt. Simpson's collections from these rocks, including the coal at Sulphur Creek, Wyoming, to the Cretaceous.‡ Prof. Marsh had also referred coal beds on Brush Creek, Wyoming, to the Cretaceous in 1871.‖ Two fossils, and only two, from one part of the formation mentioned, were referred by Mr. Meek to the Tertiary, but this was from a misapprehension in regard to the locality and stratigraphical position of the Hallville coal mines, which, we are informed, he had then never visited, and supposed to be located 20 to 30 miles farther eastward, and at a much higher horizon; and these fossils, the only species found at Hallville, are just such forms as might be either Tertiary or Cretaceous, and are nearly allied to Tertiary species of Europe.

2. *Decaisne's Monograph of the Genus Pyrus.*—A volume of Decaisne's great work—or rather of one of his great works—*Le Jardin Fruitier du Muséum*, un Iconographie de touts les Espèces et Varietes d'Arbres Fruitiers cultives dans cet Établissement, &c.

* Hayden's Report of 1871, p. 375. † King's 4to Report of 1870, p. 461.
‡ Proc. Ac. N. Sci., Philad., 1860, p. 130. ‖ This Journal, March, 1871, i, 195.

(produced in first-rate style by Firmin Didot Frères), devoted to the genus *Pyrus*, is now before us. It is a complete monograph of the species of this genus, taken in its restricted sense, illustrated by figures of the wild types, and also of the cultivated races of those cider-pears known in France under the name of *Sauger*. There is a list of the cider-pears cultivated in the different provinces of France, a general alphabetical catalogue of all the published varieties of pears, and a table in which the synonyms are referred to the names severally adopted. The other volumes, and the illustrations of the edible varieties of pears, may have more interest for the horticulturist. But the present attracts the special attention of the scientific botanist.

As stated in the Introduction, Prof. Decaisne entered upon his great undertaking more than twenty years ago, when, in the year 1850, he became the Professor of Culture. He cites the instructions under which the separate collection of fruit-trees was constituted, and the professor of culture was charged with its management, and was directed to bring together all the known varieties, with all their names, "afin d'établir une uniformité de nomenclature necessaire pour toutes les parties de la République." This is a decree of the National Convention, June 10, 1793. The collection which Decaisne has so diligently and acutely studied actually dates from the year 1792, when the fruit-garden of the Chartreux of Paris was broken up, and two trees of each variety transported to the Jardin des Plantes. In 1793 it contained 185 varieties. In 1824, when Thouin died, there were in it 265 varieties of pears alone; it has now more than 1400 varieties of this fruit. It is interesting and important to know that the collection still preserves the greater portion of the very types described a century ago by Duhamel. For seven years Prof. Decaisne studied the incomparable collection under his charge, making drawings and analyses, in which he is so skillful, and an herbarium of their flowers and foliage, before he commenced the publication of the *Jardin Fruitier du Muséum*, which he is now bringing to a close.

As to giving a correct nomenclature and available characters, this is difficult enough, as all botanists know, for the species themselves (which must needs have, or be assumed to have, real distinctions) in any large genus, such as *Quercus*, *Rosa*, *Rubus*, and the like; how much more difficult, even to impossibility, it must be in the case of cultivated varieties, of ever increasing numbers, usually named without system, sometimes of mixed origin, and often too like each other to be distinguished by any available descriptions. Here colored plates are a necessity; and those of this great standard work, upon which no pains have been spared, leave little to be desired that art can supply.

In France alone they count about 800 sorts of pears; the origin of most of them is unknown, and many are undoubtedly very ancient. Indeed, according to Jordan and his school these differences are primitive, and the so-called races and varieties, both of wild and cultivated plants, represent so many closely related

species. But M. Decaisne, not content with the *reductio ad absurdum* of having about 2000 species of pears to be dealt with, proceeded to an experimental demonstration of the variability of the cultivated races. He sowed the seeds from four very distinct varieties in 1853, the *Poire d'Angleterre*, the *Bosc*, the *Belle Alliance*, and the *Cirole*. Of the last the four trees raised bore fruit of four different forms. From the *Belle Alliance* he obtained, in this first generation, nine new varieties, none of them representing the parent, neither in the form, size, color, nor even the time of ripening of the fruit. The *Bosc* equally produced new varieties. Of the *Angleterre* nine trees produced as many new forms, one of them a winter-pear similar to the *Saint Germain*, another apple-shaped fruit identical with one which was raised from the *Belle-Alliance*. On plate 33, Decaisne gives figures of six different pears raised from the *Angleterre*. These results even led him to doubt the cases cited by Darwin of the reproduction of certain pears from seed. He insists, moreover, that very bad fruits may be raised from choice cultivated pears, and that good varieties may be obtained from the seeds of wild pears. The latter is not what one would expect in the first generation.

Our author proceeds to state that the trees raised from seed taken from the same fruit differed, not merely in their fruits and in the time of ripening, but no less in their flowers and in the form of the leaves. Some were thorny, others thornless; some produced slender shoots, others thick and stout shoots, &c. It is worth noticing, however, that no mention is made of any precautions to prevent cross-fertilization of the flowers from which the seeds planted were derived, which might have influenced the product through the now well-ascertained influence of the pollen upon the pericarp. We perceive, however, that he would regard this as unimportant, since pear-varieties are of the lowest grade, incapable of propagating fruit by close-fertilization, and, therefore, wholly unlikely to impress by their pollen any characteristic upon the pericarp of another variety.* A large part of the Introduction is occupied with further evidence that the Pear-trees of cultivation are all of one species, from which have proceeded six races, completely fertile *inter se*, and varieties *ad infinitum*. In this respect the Pear-tree has but followed the example of most fruit and fruit-trees, and of the Grains, &c., which had apparently diverged into

* Yet the apple, which is in the same case, does so. An interesting instance of this kind lately came under our notice, an apple from a spitzenberg tree, one-half (at least as to the surface), *spitzenberg* the other half *russet*. A tree of the latter fruit stood about 200 yards off. Several cases of this sort are known, in which, as in this, the division is into two exactly equal parts of the circumference, and the line of demarcation abrupt. This is quite unexpected, as the Secretary of the Smithsonian Institution, who sent us the fruit, remarked; for as the styles and carpels were five, we should have expected the division to be into fifths, and according to the number of the stigmas which were acted upon by the foreign pollen. It is, moreover, to be noticed that the action of the pollen in this case is manifest upon what is morphologically the calyx, not upon the pericarp. The apple we refer to was grown in the orchard of William Wicksham, of Washington Co., Penn. A. G.

races, or distinct but closely related types, in very early times, and those under cultivation have themselves varied and subdivided more and more. Finally, M. Decaisne maintains, seemingly with good reason, that to combine into one genus the Apple, Pear, Quince, Sorb and Mountain Ash, as done by Linnæus and followed by the latest authorities, is to misconceive the laws of the natural system; that "to unite generically these plants, which differ in the character of their wood, the vernation of their leaves, their inflorescence, the æstivation of the corolla, and the structure of their fruit," logically leads to the combination of all *Pomaceæ* into one genus. He accordingly restricts the genus *Pyrus*, or (restoring the classical orthography) *Pirus*, as did Tournefort and Jussien, to the Pear proper. To the organography of this restricted genus, from the wood to the embryo, a full chapter is devoted. In the course of this the relative systematic value of characters observed is brought out. He notes that the vernation of the leaves is involute in *Pyrus*, but not in *Cydonia*, *Mespilus* and *Aria*; that the cottony-leaved varieties, no less than the smooth ones, are glabrous in the seedling stage; that all varieties of the common Pear blossom at Paris whenever, in the month of April, the mean temperature reaches about 10° Centigrade, without perceptible difference between the earliest and the latest-ripening varieties; that the æstivation of the corolla is convolute in *Cydonia*, but imbricate in the Pear, although ordinarily quincuncial in other *Pomaceæ* (but in the two diagrams of Pear-flowers on Plate A, one has the quincuncial, i. e., in our view typically imbricative æstivation of the corolla; in the other, there is only one wholly outer and one inner petal—a combination of the quincuncial and the convolute modes which often occurs, but which need not be taken as the type of imbrication); that there are two types as to size of the corolla in the common Pear, the smaller flowered type comprehending most of the cultivated varieties; that the odor of Pear-blossoms is rather disagreeable than otherwise, in contrast with those of *Malus*, which are sweet-scented. Moreover, the anthers in the Pear genus are tinged with violet; those of the Apple genus are yellow.

As to the morphology and development of the gynæcium, Decaisne reproduces in full the note which he published in the Bulletin of the Botanical Society of France in 1857. From his investigation it appears that the five carpels in their early development are free and distinct in the concave center of the flower; that at a later stage, when the concave receptacle has become much deeper, a cellular tissue develops from its base and inner face, moulds itself around and over the carpels, so as separately to envelope them, except at their inner angle, while it carries up the petals and stamens, and forms the perigynous disk upon which they are inserted; this forms the *core* or central part of the flesh of the fruit, which we have always regarded as receptacle, never ceasing to protest against the still prevalent notion (continued in the latest general works), that the cartilaginous or bony "cells" are "endocarp." But, while we were disposed to regard the

whole exterior flesh as calyx, Prof. Decaisne (no doubt correctly) regards it as mainly receptacle or axis,—an *hypanthium* which in common pears is largely a hypertrophy of the peduncle, after the fashion of *Anacardium*.

In the proper Pear genus, the ovules never exceed a single pair; this should therefore enter into the generic character.

"Theophrastus had already remarked that the older the Pear tree, the more prolific, and every day's experience confirms the justice of this observation." The gritty grains or lignified cells which are so abundant in the flesh of many sorts of pears are not wholly absent from any of them. To them is due the roughish surface of the skin, as contrasted with the smooth skin of apples. It is curious to remark that Meyen, in his Pflanzen-Pathologie, considered the gritty grains to be a disease which attacked pears and quinces.

It appears that pear-growers are able to produce fruits of abnormal size by supporting the growing pear from underneath, instead of allowing it to hang on the peduncle. M. Decaisne has seen *Poires de Livre* of a kilogram, *Goulu-Morceau* of 600 grams, and a *Chaumontel* of 700 grams weight, produced in this way.

The testa of all Pomaceous seeds is smooth and more or less mucilaginous, except of a *Photinia*, in which it is reticulated. The cotyledons are accumbent relative to the rhaphe, except in a *Photinia*, *Cotoneaster*, *Pyracantha* (*Cratægus Pyracantha* Pers.), and *Eriobotrya*, in which they are incumbent. At first there is a thin layer of albumen, which disappears at maturity of the seed.

Pears are commonly grafted upon a Quince stock. But it is confidently asserted, and generally supposed, that there are more than forty varieties which absolutely refuse this union, and which are therefore managed by surgrafting upon a pear stock of a proper sort which has itself been engrafted upon the quince. But, as Prof. Decaisne remarks, horticulturists are too apt to generalize their impressions and to limit nature to the narrow horizon of their own practice. Upon the first trial of the experiment under his own observation, he succeeded with twenty of these antipathetic varieties without the least difficulty; but some (among which are the *Clairgeau* and the *Bosc*) obstinately refuse to unite with the quince stock. He naturally discredits the assertion made by Cabanis and by Downing (cited by Darwin), that when certain pears are grafted on the quince, their seeds produce trees of types different from those which they do when they are raised upon a pear stock. Decaisne found, as already stated, that pear-seeds produce indifferently new varieties in any case; that these varieties are not at all fixed into races. He regards as wholly unproven all the assertions that the fruit is ameliorated or in any degree altered by grafting upon a quince or any other stock. He records a very exceptional instance in which the antipathy of the pear to the apple as a stock was so far overcome that the graft survived at least six years, but without vigor, and bore fruit; still this antipathy confirms the generic difference between *Pyrus* and *Malus*.

We must pass over the sections on the diseases of the pear, and the parasitic plants and insects hurtful to it; while as to that on the classification of the pears of cultivation, we may mention merely the conclusion, which is, that a natural classification of pears is thus far an impossibility; and that in practice nothing better can be done than to follow the example of the older pomologists, who arranged them according to the period of ripening. A general list of the adopted names of the published varieties of cultivated pears, alphabetically arranged, fills four pages of the volume. A list of their synonyms, in which each is referred to the adopted name, fills over 12 pages! Then follows a list of pears classed according to the period of maturing, and in which the best varieties are designated.

Finally comes a botanical monograph of the genus *Pyrus*, with a full generic character, and descriptions and figures of the *races*, as he would term them, considering as he does all known forms of the restricted genus as a single and very polymorphous species.

The six races are: 1. The Celtic, *Proles Armoricana*, of three quasi-species, *P. cordata*, *Boissieriana* and *longipes*. 2. The Germanic, *Proles Germanica*, or *Pyrus communis*, including our common pears, both pear-shaped and apple-shaped, "both forms being often met with upon the same tree." Under this head Prof. Decaisne gives some interesting pages upon the history of the cultivation of pears in France, which cannot be ancient, and of cider (perry) as a drink. It appears that it took the place of beer in the north of France in the fifteenth century or later, and is now giving way to wine and perhaps beer again; and that pears would have disappeared before this from a part of Normandy, were it not that *they are carried in immense quantities to Epernay, where they are used in the manufacture of champagne.* 3. The Hellenic Race, which comprises *P. parviflora* and three other subspecies. 4. The Pontic Race, *P. salicifolia* and its allies. 5. The Indian Race, *P. Pashia* and its relatives. 6. The Mongolian Race, *P. Sinensis* and its varieties. As one turns over the excellent plates one can hardly be persuaded that such extremely diverse forms can practically be regarded as of one species.

A list of the species remanded from Pyrus to other genera shows that the result of our author's prolonged and sagacious study is to increase the genera about as much as he diminishes the species of the Linnæan Pyrus. A. G.

3. *Botanical supplement to the fifth Annual Report of the U. S. Geological Survey of the Territories for* 1871; by M. Lesquereux.—This supplement by M. Lesquereux (prepared in May, 1872) contains the description of a number of species of fossil plants from specimens which were received when the report was already in print. The essential points marked by the author as resulting from the examination of these plants are as follows:

1st. It adds to our list of fossil species of the Tertiary 20 new forms, and describes 21 others known already from the Miocene of Europe, but not as yet observed in our Tertiary flora. The number of its species is thus increased to 231.

2d. It fixes the geological horizon of three localities in different stages of the Tertiary and marks the location of a group of specimens of as yet unknown origin.

3d. It more distinctly points out the relation of some important strata for ascertaining contemporaneity or difference of age.

4th. It indicates more positively modifications in the characters of the Tertiary flora of the North American continent, according to climatic differences at different degrees of latitude, and at the same time, it recognizes identity of the characters of this vegetation at wide distances under the same latitude.

5th. It shows a more intimate relation between the present flora and that of the Tertiary by the discovery of new types identical in both.

This relation is especially indicated by the fossil plants of Green-River station, which from their more recent facies are referable to the Upper Miocene. Among species of *Salix*, *Myrica*, *Ilex*, and *Rhus*, whose representatives are intimately related to species of our time, the fossil flora of Green River has an *Ampelopsis* and a *Morus* which by their marked affinity indicate in the Tertiary the origin of our now so predominant and widely distributed Virginian Creeper and Red Mulberry.

## III. Astronomy.

1. *Elements of Alceste;* by Prof. C. H. F. Peters. (Editorial Correspondence, dated Litchfield Observatory of Hamilton College, Clinton, N. Y., Nov. 9, 1872).—The following elements of *Alceste* (124) have been computed from observations of Aug. 23, Sept. 22, Oct. 21:

Epoch: 1872·0 Berlin mean time.

$M_0 = 22^\circ\ 28'\ 1''{\cdot}94$. $\varphi = 4^\circ\ 26'\ 58''{\cdot}80$.

$\pi = 346\ 59\ 47{\cdot}82 + 50''{\cdot}236, t$. $\mu = 834''{\cdot}47$.

$\Omega = 186\ 19\ 39{\cdot}65 + 52{\cdot}573, t$. $\log a = 0{\cdot}419063, t$.

$i = 2\ 55\ 47{\cdot}48 - 0{\cdot}455, t$—counting $t$ in Julian years from 1872·0.

## IV. Miscellaneous Scientific Intelligence.

1. *Analysis of the Meteoric Iron of Los Angeles, California;* by Dr. C. T. Jackson, State Assayer of Massachusetts. (Communicated to one of the editors).—Having received from Mr. E. N. Winslow a slice of the recently found meteoric iron of Los Angeles, I have made a chemical analysis of it, which I now communicate to you. The original mass is stated to have weighed 80 lbs. The slice I received weighed 30 grams. Its specific gravity is 7·9053.

It shows, when acted upon by dilute nitric acid, innumerable scales of Schreibersite, but not the usual figures.

In the chemical analysis I found in the insoluble matter, on reduction by blowpipe, a minute globule of tin. The iron was separated by succinate of ammonia and the nickel by pure potassa.

The following are the results of the analysis per cent.

| | |
|---|---|
| Metallic iron, | 80.74 |
| Metallic nickel, | 15.73 |
| Metallic tin, | 0.01 |
| Phosphorus and other undetermined matters, | 3·52 |
| | 100.00 |

This analysis, although not quite complete, shows beyond doubt the meteoric nature of the Los Angeles iron.

Boston, Sept. 26, 1872.

2. *Tables and Diagrams relating to non-condensing Engines and Boilers;* by W. P. TROWBRIDGE, Prof. of Dynamic Engineering, Sheffield Scientific School of Yale College.—This publication gives the results, in tables and diagrams, of a laborious and costly series of experiments made at the Novelty Iron Works, New York, during the time when Prof. Trowbridge was Vice President of the Novelty Iron Works, to determine practically the expenditure of steam for different degrees of expansion in the non-condensing engine. The experiments occupied several months, and were made with an engine and apparatus constructed especially for the purpose.

An extended list of engines is published, embracing powers from 5 horse-power to 350 horse-power, amounting in number to over 1000, and embracing all combinations of pressure of steam, speed of revolution, and degree of expansion. For each engine in the list the amount of water or steam required per hour for each horse-power is given and also the total expenditure of steam per day, and the cost of the power per year.

Corresponding with this list, a table of steam boilers, is given with the amounts of water which each boiler will evaporate in one hour. A discussion of the questions of horse-powers of boilers, and of boiler explosions, safety valves, &c., is added. A new and very simple improvement to the safety valve, suggested by the author, is also given.

The work throughout is intended to be practical but at the same time it adds to existing knowledge of the subjects discussed the results of the latest reliable experiments.

3. BLOXAM (CHAS. LOUDON): *Chemistry, Inorganic and Organic, with Experiments.* Second Edition (American reprint). Lindsay & Blakiston. Philadelphia, 1872. 8vo, pp. 666.—Bloxam's Chemistry has been a familiar and valued laboratory and lecture-room companion for several years. The present edition (July, 1872), differs from its predecessor in being adapted to the atomic notation, which has required a considerable recast of the old text. The illustrations of apparatus are very good, embracing many which are original with the author. The teacher finds on almost every page some experimental suggestions which enable him to give force and attractiveness to his prelections. The text is written in simple language, as devoid as possible of technicalities, and designed to lead the learner on by easy steps in a knowledge of chemical principles. The author's position as a

lecturer in the Royal Military Academy at Woolwich has very properly led him to give more than usual prominence to those subjects which are of interest to the military student, as, combustion and fuel, gunpowder, gun-cotton and other explosives, the process of bread making, glass, pottery, the chemistry of building materials, and kindred subjects.

4. *Report of the Mt. Uniache, Oldham, and Renfrew Gold Mining Districts*, with Plans and Sections; by HENRY YOULE HIND, M.A. Made under instructions from the Hon. Commissioner of Public Works and Mines. 62 pp. 8vo. With maps and sections. Halifax, N. S., 1872.

5. *Astronomical Engravings, by the Observatory of Harvard College.*—Fourteen plates, announced on page 243 as in course of preparation for publication, have been issued. They are views of Jupiter, craters of the moon, the sun's protuberances, the sun in the total eclipses of August, 1869, and of December, 1870, etc.; and are very beautiful. The series will consist of thirty plates. Subscriptions are solicited; the charge for the series is ten dollars.

## OBITUARY.

JOHN F. FRAZER, Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, died on the 12th of October, in the sixty-third year of his age.

Prof. Frazer was born in 1809 in the vicinity of West Chester of this State. His grandfather, General Persifor, took a prominent part in the leadership of the American Revolutionary troops at the battle of Brandywine. In his early youth he attended the celebrated military academy of Captain Parrott, and here acquired the military taste that clung to him through life, which exhibited itself in his membership for a long time of the 1st City Troop. He marched with this organization to suppress the riots of '44 in this city, and also was present as a soldier in the celebrated "Buck-shot" war at Harrisburg. He was educated and graduated in the institution whose chair of physics he so nobly filled for thirty years. During his student days he was the favorite scholar of Dr. Samuel Wylie, professor of Latin and Greek in the old university, and spent some time in his family after graduation. He assisted Dr. Robert Hare in the laboratory, and subsequently became a most intimate friend and valued companion of that distinguished contemporary and rival Prof. Silliman, of Yale, the two being the most illustrious chemists of their day. At the same time Prof. Frazer was also the assistant of Prof. Alex. Dallas Bache, of the Coast Survey, during his observation at the then new physical observatory for magnetic phenomena at Girard College. In both these instances he was a voluntary assistant.

In 1836 he became the assistant of Professor Henry D. Rogers, one of the first geologists of the country, and Professor Booth, of the Mint, in the geological survey of Pennsylvania, ordered by the Legislature. Professor Frazer resigned this position as First Assistant State Geologist after one year, and then took up the study of law in the office of Hon. Wm. M. Meredith. Having

concluded his law studies, he realized the cherished dream of his youth in his elevation under the regime of Provost Ludlow to the chair of natural philosophy and chemistry in the academical department of his *Alma Mater*. This position he filled with the highest honor to his college and to himself for over thirty years.

Soon after assuming the gown of the professor, he became the able editor of the well-known scientific *Journal of the Franklin Institute*. He resigned his editorial duties in 1866 in consequence of ill health, and crossed the ocean for a two years' tour on the Continent. He resided in Naples for the first year, and then traveled extensively until he returned to his college duties in the summer of 1868, Professor Morton, now president of the Hoboken Institute, having occupied his chair while away.

Professor Frazer's study was the center of physical science in the city, and was the rendezvous for the most distinguished scientists in this and foreign countries when they visited Philadelphia. He was known throughout Europe by his constant writings as most accurate in science and universally learned in his department. He was a member of the Academy of Natural Sciences of this city, and also one of the four secretaries of the mother society of science in this country—the American Philosophical Society of this city.

Although holding the chair of chemistry and natural philosophy, his favorite science was that of mathematics as applied to mechanics. This branch brought out his close study and keen appreciation of all the basic laws of mechanics, and raised him to a deservedly high rank as a transcendental mechanician.

Professor Frazer had a remarkably genial disposition, was full of life and humor among his fellows, and though brusque in manner and decided in his intercourse with his pupils, he was generally liked by all who came in contact with him. He was bold and outspoken in all his feelings, being an ultra Democrat—in its original sense—in his principles. His self-sacrificing spirit was a remarkably noticeable point in his character, and, in addition to the respect and confidence which his great learning compelled, gained him the love of all whom he could benefit in his walk through life.—*The Press, Philadelphia*, Oct. 14.

**Notes of an Ornithological Reconnoissance of portions of Kansas, Colorado, Wyoming and Utah**; by J. A. Allen, 72 pp. 8vo. July, 1872. Being No. 6, vol. III, of the Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass.

**Problem of Rotary Motion presented by the Gyroscope, the Procession of this Equinoxes and the Pendulum**, by Brevet Maj-Gen. J. G. Barnard. 48 pp. 8vo. No. 240 of the Smithsonian Contributions to Knowledge.

**Intermembral Homologies. The Correspondence of the Anterior and Posterior Limbs of Vertebrates**; by Burt. G. Wilder, M.D., Prof. Comp. Anat. and Zool. in Cornell Univ., Ithaca, N. Y. 88 pp. 8vo. From the Proceedings of the Boston Soc. Nat. Hist., vol. xiv.

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ERRATA.

Vol. ii, p. 180, *for* "(near Chatham Four-corners)" *read* "(near Galesville, Washington Co., N. Y.)."

Vol. iv, page 53, 8th line from top, *for* "particularly" *read* "partially."
" " 123, last line, *for* "*Mastodon anceps*," *read* "*Tinoceras anceps*."
" " 185, line 18 from top, for *district* read *distinct*.
" " 242, " 10 " " for *tilted* read *silted*.
" " 323, 20 lines from bottom, for "very small" read "narrow."
" " " 13 lines from bottom, for "but five" read "six."
" " 370, 19 lines from foot, before "Trenton," insert "Quebec."
" " 389, line 10 from top, for *ventral* read *nodal*.
" " " " 12 " for $UT_8$ read $UT_2$.
" " 451, " 9 " for 500 read 300.

# APPENDIX.

*Summation of Series by Approximative Fractions.*
By R. J. Adcock.

This method of summation of series consists in putting the series into a continued fraction by common algebraic division, and then finding its approximating fractions until one is found which gives the sum of the series either approximately or entirely.

*Ex.* Find the sum of the series $a + ar + ar^2 + ar^3 +$ &c.

$$\begin{array}{l|l} a+ar+ar^2+ar^3+\&c.) & 1 \quad \left(\frac{1}{a} \\ \hline 1+r+r^2+r^3+\&c. & \\ -r-r^2-r^3-\&c.) & a+ar+ar^2+ar^3+\&c. \left(-\frac{a}{r} \\ & a+ar+ar^2+ar^3+\&c. \\ \hline & \text{no remainder.} \end{array}$$

Then

$$a+ar+ar^2+ar^3+\&c. = \frac{a+ar+ar^2+ar^3+\&c}{1} = \frac{1}{\frac{1}{a}+\frac{1}{-\frac{a}{r}}} = \frac{a}{1-r},$$

which is the entire sum of the series and the exact algebraic expression from which it is derived. To find the sum of $n$ terms, it is to be observed that beginning with the $(n+1)$th term, the series is $ar^n+ar^{n+1}+ar^{n+2}+$ &c. = by the same process, $\frac{1}{\frac{1}{ar^n}+\frac{1}{-a r^{n-1}}} = \frac{ar^n}{1-r}$. Hence $a + ar + ar^2 + ar^3 + \ldots . ar^{n-1} =$ $\frac{a}{1-r} - \frac{ar^n}{1-r} = \frac{a(1-r^n)}{1-r}$, the common formula for the sum of $n$ terms of a geometrical series.

The superiority of this method of summation over others is:

First, its comparative simplicity, on account of which it is worthy of a place in common algebra.

Second, it is more generally applicable than any method known to me.

Third, the facility with which it gives the entire sum of a series when capable of being expressed in finite terms, and the rapidity with which it approximates to the sum of those capable of being expressed only approximately.

## *Equilibrium of a fluid mass in the form of an ellipsoid rotating about its shorter axis;* by R. J. ADCOCK.

A FLUID mass, in the form of an ellipsoid, rotating about its shortest axis, under the action of the attraction of its own particles and their centrifugal forces, is in equilibrium; and this is the only form of equilibrium.

In my work on Gravitation, and in a paper* presented at the Dubuque meeting of the American Association for the Advancement of Science, it is shown that the ellipsoid is a form of equilibrium, and the expression found, for the force at any point of its surface. It remains, then, only to demonstrate the last part of the proposition, that the ellipsoidal form is the only one of equilibrium, under the given conditions.

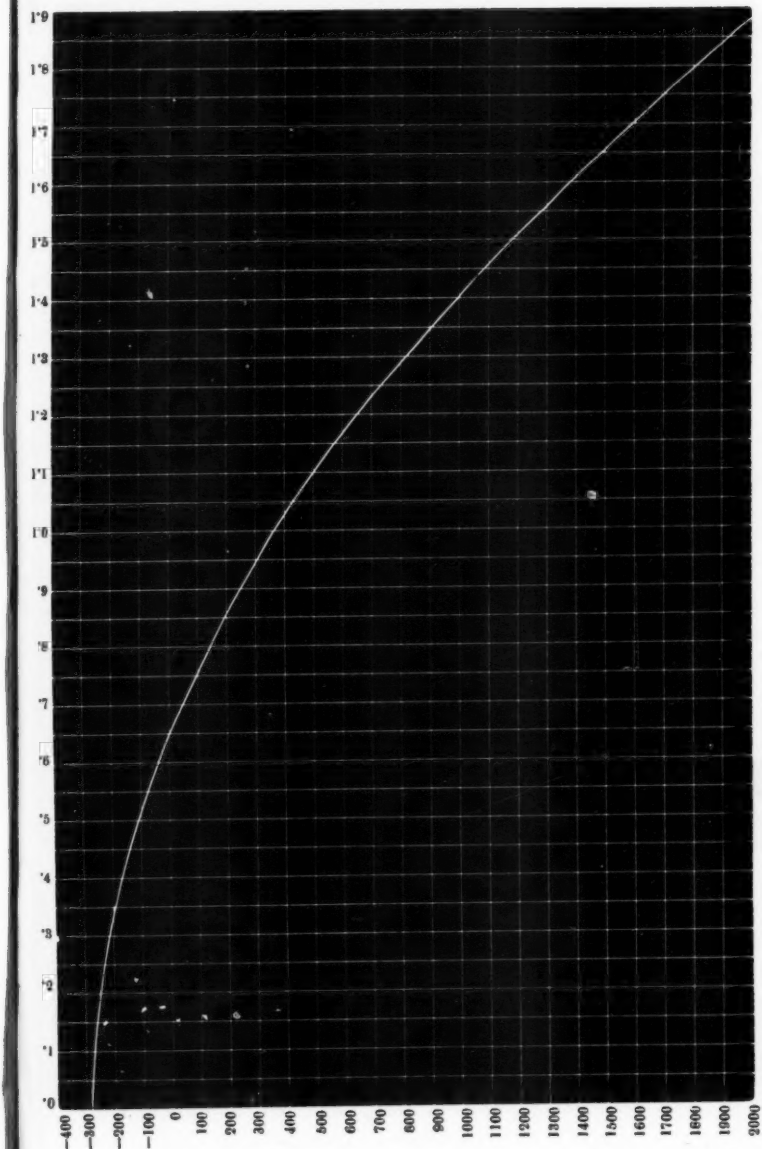
The proof of this depends upon two propositions.

*First.* The attractions of two similar bodies for any two exterior points similarly situated, are as their similar dimensions, and consequently as the distances, of the attracted points from the centers of the attracting bodies.

*Second.* Ellipsoidal shells which have their outer and inner surfaces concentric, similar, and similarly placed, are the only ones which have their attractions for an interior point zero. This follows from formula (1) of Gravitation, and the proposition that these shells are the only ones which have the distances through them in opposite directions from any point within equal, as is shown in problem 4th, page 211, Senate House Problems, by Ferrer and Jackson, Cambridge, 1850.

Then when any rotating fluid mass is in equilibrium under the attractions of its own particles and their centrifugal forces, any one of similar figure and same density and rotating in the same time, is also in equilibrium. For the ratio and directions of the attractions and centrifugal forces are the same in both, and therefore their resultants as their similar dimensions. Hence, if the less one be enveloped by a shell which is the difference of the two, the entire figure thus formed, which is every way equal to the greater, will be in equilibrium. Hence, the attraction of the enveloping shell has no effect on the interior mass, a property just shown to belong only to the ellipsoidal shell whose outer and inner surfaces are concentric, similar, and similarly placed. Hence, the ellipsoidal form is the only one, under the given conditions, of equilibrium.

* This paper and the one presented at the Dubuque meeting of the American Association are to be inserted in my work on Gravitation, which will then contain the only correct formula ever published for determining the figure of the earth on the hypothesis of fluidity, whether that figure be an ellipsoid of three unequal axes or two.

Lenox, Warren County, Illinois, Sept. 12th, 1872.

AM. JOUR. SCI., IV, Vol. IV, Plate V. 1872.